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TRANSACTIONS OF THE AMERICAN CERAMIC SOCIETY

CONTAINING THE PAPERS AND DISCUSSIONS OF THE FOURTH ANNUAL MEETING HELD AT CLEVELAND, OHIO FEBRUARY 10-11-12, 1902

Vol 4

PUBLISHED BY THE SOCIETY
SECRETARY'S OFFICE, COLUMBUS, O.
OCTOBER 1902

TRANSACTIONS

OF THE

AMERICAN CERAMIC SOCIETY

CONTAINING THE PAPERS AND DISCUSSIONS

OF THE

FOURTH ANNUAL MEETING

HELD AT

CLEVELAND, OHIO,

FEBY 10-12, 1902.

The Society is not, as an organization, responsible for the statements of fact or opinion expressed in its papers and discussions.

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FOR THE YEAR ENDING FEBRUARY, 1903.

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Term Expires, 1903.

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MADDOCK, JOHN, Trenton, N. J,	John Maddock & Sons.
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RIX, WILTON PARKER, 84 Liverpool Road, Newcastle, Staffordshire, England.	Consulting Ceramist.
ROESSLER, FRANZ. 100 William St., New York, N. Y.	Vice Pres,, and Supt., Roessler & Hasslacher.
RUSSELL, JOSEPH, 1802 Queen St., East, Toronto, Canada.	Brick Manufacturer.
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SIMPKINS, RALPH, St. Louis, Mo.	Manager Union Press Brick Wks.
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YATES, ALFRED, Johnsonburg, Pa.	Johnsonburg Vitrified Brick Co.

RULES.

OBJECTS.

The objects of the American Ceramic Society are to promote the arts and sciences connected with Ceramics by means of meetings for social intercourse, for the reading and discussion of professional papers, and for the publication of professional literature.

MEMBERSHIP.

The Society shall consist of Honorary Members, Members and Associates.

Honorary Members must be persons of acknowledged professional eminence, and shall not exceed five in number.

Members shall be persons competent to fill responsible positions in Ceramics and have suitable qualifications.

Associates shall include persons interested in Ceramics and the allied arts.

Honorary Members shall be proposed by at least five Members, approved by the Council, and receive at least 90 per cent. of the votes cast by letter ballot at the annual meeting.

Members and Associates shall be proposed by at least three Members or Associates, approved by the Council, and receive at least 75 per cent. of the votes cast by letter ballot. A candidate for admission must make application on a form prepared by the Council which shall contain a written statement of his age, professional experience, and that he will, if elected, conform to the laws, rules, and requirements of the Society.

All Honorary Members, Members and Associates shall be equally entitled to the privileges of membership, except that only Members shall be entitled to hold office and to vote. Applicants for a change in grade of membership shall conform to the requirements of a new applicant.

Any person can be stricken from the membership of the Society on the request of five or more Members, on the recommendation of a majority of the Council, if he fails to resign on the advice of the Council. Such person, however, shall first be notified of the charges against him, and be given a reasonable time to appear before the Council, or present a written defense, before final action is taken by the Council.

DUES.

Honorary Members shall be exempt from all dues.

The initiation fee of Members shall be ten dollars, and of Associates five dollars, which if not paid within six months after election, will render the election void.

The annual dues for Members will be fixed by the Council, but shall not exceed five dollars per year.

The annual dues for Associates will be fixed by the Council, but shall not exceed four dollars per year.

Any Member or Associate in arrears for over one year may be suspended from membership by the Council until such arrears are paid.

OFFICERS.

The affairs of the Society shall be managed by a Council, consisting of a President, Vice President, Secretary, Treasurer, and three Managers, who shall be elected from the members at the annual meeting, and hold office until the adjournment of the meeting at which their successors are elected.

The President, Vice President, Secretary and Treasurer shall be elected for one year, and the Managers for three years; and no President, Vice President, or Manager shall be eligible for immediate re-election to the same office.

The duties of all officers shall be such as usually appertain to their offices, or may be delegated to them by the Council or the Society; and the Council may at its discretion require bonds to be furnished by the Treasurer.

Vacancies in any office shall be filled by appointment by the Council, but the new incumbent shall not thereby be rendered ineligible to re-election at the next annual meeting to the same office. On the failure of any officer to execute his duties within a reasonable time, the Council, after duly warning such officer, may declare the office vacant, and appoint a new incumbent.

A majority of the Council shall constitute a quorum; but the Council shall be permitted to carry on such business as it may desire by letter.

ELECTIONS.

At the annual meeting, a nominating committee of five Members, not officers of the Society, shall be appointed, and this committee shall send the names of nominees to the Secretary at least 60 days before the annual meeting, who shall immediately forward the same to the Members. Any other five members may also present the names of any candidates to the Secretary, provided it is done at least 30 days befor the annual meeting. The names of all candidates, provided their assent has been obtained, shall be placed on the ballot without distinction as to nomination by the regular or an independent nominating committee, which shall be mailed to every member, not in arrears, at least 20 days before the annual meeting.

The ballot shall be inclosed in an inner blank envelope, and the outer envelope shall be endorsed by the voter, and mailed to the Secretary for collection. The blank envelopes shall be opened by three Scrutineers appointed by the Chair at the annual meeting, who will report the result of the election. A plurality of the votes cast shall elect.

MEETINGS.

The annual meetings shall take place at such time and place as the Council may decide, at which time reports shall be made by the Council, Treasurer, and Scrutineers of election, and the accounts of the Treasurer audited by a committee of three appointed by the Chair.

Other meetings may be held at such times and places during the year as the Council may decide, but at least 20 days' notice shall be given of such meetings.

Seven Members shall constitute a quorum at any regular meeting, and a majority shall rule, except where otherwise specified.

The order of business at the annual meeting shall be:—

1. Reading of Minutes of last meeting.
2. Reports of the Council and Treasurer of the Society.
3. Announcement of Election of Members.
4. Announcement of Election of Officers.
5. Appointment of Nominating Committee.
6. Old Business.
7. New Business.
8. Reading of Papers.

PUBLICATIONS

The Council shall act as a Publication Committee, and decide as to what to publish. The publications of the Society shall be sent to all Members and Associates not in arrears. The Secretary will furnish each author with reprints of his papers at cost price, provided due notice is given that reprints are desired.

The Society is not, as a body, responsible for the statements or opinions expressed in its publications.

PARLIAMENTARY STANDARD.

Roberts' "Rules of Order" shall be the parliamentary standard on all points not covered by these rules.

AMENDMENTS.

These rules may be amended at any regular meeting by a two-thirds vote of a letter ballot at the subsequent annual meeting provided a written notice of such proposed change is sent to each Member at least 30 days before said annual meeting. Said proposed amendments shall be printed on the ballot for officers and counted by the same Scrutineers.

PUBLICATIONS.

The publications of the Society are as follows:

DESCRIPTION OF VOLUME.	Price to Members.	Price to Others
Vol. I. Transactions for 1899, 110 pages, bound in paper,	$ 0.50	$ 4.00
Vol. II. Transactions for 1900, 278 pages, bound in paper,	2.00	4.00
Vol. III Transactions for 1901, 280 pages, bound in paper,	1.00	4.00
Vol. IV. Transactions for 1902, 300 pages, bound in paper,	1.50	4.00
Manual of Ceramic Calculation (contained in Vol. II as a part thereof,) 86 pages, bound in paper,	1.00	1.00
The Collected Writings of Dr Hermann August Seger, Volume I. Contains (A) Treatises of a general scientific nature, (B) Essays relating to Brick and Terra Cotta, Earthenware and Stoneware, and Refractory Wares. Pages 552. Bound in cloth.	7.50	7.50
The Collected Writings of Dr. Hermann August Seger, Volume II. Contains (B) Essays on Whitewares and Porcelain. (C) Travels, Letters and Polemics. (D) Uncompleted works, and extracts from the archives of the Royal Porcelain Factory. Pages 590 (est). Bound in cloth.	7.50	7.50

Every member or associate receives one copy of the Transactions free for each year for which he pays dues. He is entitled to purchase copies for the years antedating his connection with the Society at the costs indicated in the first column. All others can obtain copies at the prices listed in the second column by sending order, accompanied with check, to

STANLEY G. BURT,
Care of Rookwood Pottery Co.,
CINCINNATI, OHIO.

PREFACE.

The following pages contain the papers and discussions of the Fourth Annual Meeting, held at Cleveland, Ohio, February 10th, 11th and 12th, 1902.

In addition, a new departure has been made in this volume, in publishing a number of articles and contributions which have not been read before the Society, and, in fact, were not prepared at the date of the meeting. This policy, it is hoped and believed, will be the first step towards relieving the pressure of our meetings, in which it has grown more difficult each year to hear all of the papers and discussions offered. As the Society grows in importance and influence, it will surely reach a point where only the papers of unusual general interest will be read at the meetings, and the others will be read by title and published. The progressive intricacy of the papers already presented has shown very clearly that it is only in occasional cases that masses of technical facts can be made intelligible by reading aloud, while they readily can be comprehended when put in print.

From this point of view, therefore, as well as the number and quality of papers, it seems that this is by all means the best year's work yet accomplished by the Society. In addition, it may be said that the volume would have been improved by the addition of two other articles, one of more than general interest to brickmakers, had not the time limits urgently required that this book be sent out without delay. It is expected that one or both of these articles will still be printed and distributed as a bulletin, between this date and that of the next annual meeting.

The growth of the Society has continued rapid and good. The members seem enthusiastic, and certainly gives one evidence of this fact in maintaining their connection with very few withdrawals for any cause. The continued increase in foreign membership is noted with gratification.

The announcement of the facts concerning the publications has been given a separate page in this issue.

EDWARD ORTON, JR.,
Secretary.

COLUMBUS, O., Oct. 1, 1902.

THE PROGRESS OF THE CERAMIC ART.

ADDRESS BY THE RETIRING PRESIDENT,

CHARLES F. BINNS, ALFRED, N. Y.

It is well, in any walk of life, as we pass the posts which mark our progress, to look backward and around, that we may take a broad and comprehensive view of the field we occupy. The work done by our competitors and neighbors should concern us at least to some extent and even if we clash in rivalry, we can find evidence of mutual interest and possibly help.

Progress can be measured from three widely differing standpoints: the artistic, the technical and the commercial. In most cases the final arbitrament is found in the last, for it can be said with some measure of truth, that art and science are successful in manufacture only in so far as they prove financially sound. But this not entirely true. Beautiful work is regardless of cost and no amount of money expended can of itself make an artistic product. The fact is also self-evident that artistic wares can be made at a financial sacrifice. In other words, a product may be artistic whether the manufacture pays or not, and a profitable line is not necessarily artistic.

The same is true to a less extent of technical success. To a less extent, because science is more readily controlled than art. The artistic faculty is subtle and elusive, the scientific is exact and material. But neither is scientific and technical success to be measured entirely by money. Our best research is carried on independently of financial considerations and an improvement in quality or manufacture is not necessarily a means of making money.

Commercial success, is of course, measured by the dollar. The man who can make the most profitable ware and sell the largest quantity of it is the most successful commercial man.

From these points of view then let us take a glance over the progress of the world in recent times.

It must here be noticed that in some of the most marked advances it is difficult to say whether the artistic or the technical is the more concerned. The former is the more obvious, certainly, but the latter is fully as important. The work displayed by the National manufactory of Sevres at the Paris Exposition of 1900 was the finest exhibit of hard porcelain ever shown. Technically perfect, it was artistically fine, but, of course, would be called a commercial failure. The Royal Copenhagen factory has work which is equally fine, though very different. This factory claims a new body for plates and dishes which is much lighter than the old, without any counter-balancing disadvantage. In such a matter as this, the technical, of course, overshadows the artistic, and in fact, for reasons already stated, the artistic advance is far less easily defined. Art is a matter not of a new discovery, but of a particular expression. It is, in fact, the soul of a production, while the technical is merely the frame or body.

In illustration of this, let notice be taken of the modern English wares. There is practically no technical variation in the product of half a dozen of the leading factories and yet some are more approved than others simply because of a finer artistic sense.

The minute variation, the subtle touch which may make or mar a work of art cannot be defined, nor is it to be located by any scientific method.

Of European technical advance we have little opportunity of judging as regards coarse wares, for there is practically no competition with us in the matter. In large measure, the older methods are used and the low price of labor renders the employment of modern machinery unnecessary. Hand-moulded brick are still in the lead in England and many of the lines of work would be considered antiquated even in the slowest American brickyards.

In fine terra-cotta, France takes the lead; the productions of Muller, of Paris, stand very high both in art and technique. Germany can match the work probably, but not the spirit. It is an interesting study for those concerned as

to why most German work is artistically so poor and most French so fine. Moreover, the Germans seem to be able to make more money out of their technical knowledge than the French of their art. Take for example the flood of cheap bric-a-brac with which the Germans are covering this country. One can but admire the technical excellence of the work, the composition of body and glaze and the simplicity of manufacture, while at the same time the paucity of artistic idea is most evident. These goods are sold at such trifling cost that when the sixty per cent. duty, the freight and the packing are considered, one wonders what price the manufacturer secured, to say nothing of the amount received by the wage-earner.

The production of ceramic wares in the United States is naturally of the more immediate interest to ourselves for not only are we concerned in the manufacture, but the success of our work will of necessity diminish the importation of competing wares.

Some of our manufacturers are, happily, free from the worry of foreign competition, but they will doubtless claim special and peculiar troubles. No day-worker has a monopoly of worry, but each claims to have a special heritage of it in some individual and peculiar right. The brick-maker has the field to himself—until his neighbor's territory is reached, but he has to give his product away in order to meet his pay-roll. The maker of finer wares gets a fair price, but the stores are filled with foreign importation, and so it goes.

It is probably without question that the manufacture of building material in this country is ahead of anything elsewhere. Brick of every description, common and front, glazed and enameled, fire proofing and terra-cotta, roofing tile and wall tile as well as paving brick are made here in great perfection and the lines of drain-tile, sewer-pipe and electric conduits run them very close. In fact, where the successful product depends upon an intelligent application of engineering, the American is well to the front. Here, the artistic element scarcely appears, except in the matter of terra-cotta, but the technical and mechanical skill involved is of a very high order. Mention should be made here of

the successful manufacture of porcelain baths and sinks. But a short while ago we imported all our supply of these commodities; but now, such is the advance made, we are exporting. It should perhaps be explained that "porcelain" is only a name. The body is a refractory clay which is coated with a white slip and a clear glaze, the whole being burned at one and the same time. The result is a compact, impervious ware which, if somewhat heavy, is practically everlasting.

In terra-cotta and wall-tile great advances have been made in the matter of glazes. Every prominent factory now has a full line of tints both mat and bright, many of them very beautiful. The problems involved in each case differ somewhat in that terra-cotta is glazed almost entirely upon the unburned clay, while tile being mainly dust pressed must of necessity be fired before glazing.

In the finer branches of ceramic art we are bound, if honest, to confess a certain weakness. There are many among the ranks of the connoisseurs who ask skeptically whether America can make fine pottery, and they emphasize their doubt by saying that the necessary clay is not found in the United States. We who are engaged in utilizing native clays know better, but given the material, can we make fine pottery? In reply, we point with pride to Rookwood but are asked if a solitary example demonstrates the ability of the nation. It does, provided the necessary conditions are observed. Here allow me to digress for a moment, not in defence of Rookwood—that were quite unnecessary—but in criticism and even condemnation of the shameless copyist, who imitates body, glaze and coloring and claims something just as good at one-third the price. I hold no brief for Rookwood and my criticism would be just as severe upon any copyist. He plunders the brain and thought of the master mind, he seizes upon the external features, those that can be easily and cheaply pirated. He ignores or slaughters the soul and all that makes the work really valuable, and carries out his impudent imitation only to make money. Do we not sometimes despair for the commercial morality of our people when such things are done and approved as being smart or clever. But there is a point of

satisfaction in this cruel competition: it is, that Rookwood by virtue of her superiority of resource, can move faster than the pirates, and in this is her real safety. Fine work such as this is spontaneous and exuberant, the originator soars triumphantly upward while the slow herd of copyists flounder laboriously below.

In the manufacture of household pottery there is matter both for regret and congratulation. The wares classed by the hired girl, conveniently and comprehensively, as "dishes," are produced here in large quantities, and the matter for regret is, that they are generally so devoid of artistic merit. A glance through the advertising pages of any trade journal will reveal shapes, classed as "new" which are simply a rehash of similar things and which are evidently modeled in view of the sagger and stilt. Just what will make easily, fire safely, and hold something inside while in the kiln.

Nevertheless the manufacturers may be congratulated in that the use of American materials is extending, the bugbear of crazed goods is disappearing and the works are being run at a profit. There remains, naturally, some considerable number in the ranks of the non-progressive and even of the wilfully blind. Every now and then the ceramic world is startled by the announcement that a remedy for crazing has been found, a magic solution which, added to the dipping tub will banish forever the phantom of craze while, had the discoverer only read the current technical literature, he would long ago have found that magic is quite unnecessary and that science has already exorcised the demon and only an imaginary ghost remains to those who keep their eyes shut.

The greatest advance of recent years is in the matter of hotel china. This is a distinct outgrowth of American enterprise and skill. There had long been a dissatisfaction with French china. The ware was brittle and took long to replace. When, therefore, the American potter came forward with a tough ware — not an imitation, but a native success— he was welcomed by the hotels using large quantities of table goods. By one factory after another the problem has

been taken up and solved. Harder fires gave stronger wares, better clays gave better texture and color, improved methods gave better glazes until the American hotel china in technical quality and fitness for its purpose stands unrivalled in the world.

Once more it is laid upon me to raise the lament that we do not make fine porcelain. Hotel china, Belleek, Faience, each has its place but the lover of American ceramics cannot handle fine specimens of European wares without a pang of regret. Why is the like not made here? We have the finest clays, the best workmen, the greatest wealth. Why not then the finest porcelain? Will no patriotic manufacturer take up such a work *con amore*? Will no enthusiastic millionaire endow a small factory to remove this blot from our industrial and artistic fame? Surely, when millions are spent in collecting specimens manufactured long ago in other lands there should be a possibility of the artistic and technical skill of our own people being fostered and encouraged ro this end. Be it remembered that fine wares cannot be made for money alone, that profit must not be thought of—at least in the beginning—if a world-wide reputation is to be made, it will be apparent why our manufactures have not touched the question. For the same reason it must be evident that if we are to make porcelain which will be equal to Rookwood faience, which will compare with Copenhagen and with Sevres, it must be beneath the roof of some beneficient capitalist who will draw his profits from the pride of possession.

The members of this society may congratulate themselves that their share in the advance has not been immaterial but still more upon the fact that when a call to greater advance comes they are ready.

Much of the work has been done in the face of cavil and misrepresentation. The incredulous smile of the "practical man" still lingers around us, and there is much opposition to be lived down but the day will come, and it is not far distant, when the graduates of our schools will be sought by every plant in the country and our industry will rise to heights that are as yet unexplored.

NOTES ON THE GRINDING OF MATERIALS USED IN EARTHENWARE BODIES.

BY

ERNEST MAYER, BEAVER FALLS, PA.

The question of the degree of fineness to which the flint and feldspar used in pottery bodies shall be ground is one that does not receive in the United States the attention and care bestowed on it by European potters. It is my intention, as far as I am able, to awaken a greater interest in this subject, and show by trials submitted for your inspection, that the statements made can be amply verified.

As to the system of grinding in Europe, more particularly in England, where I gained my practical experience, let me point out a fact not generally understood, viz: That dry grinding is practically unknown—(remember, I speak of England). Although the system of dry grinding has been systematically tested and a considerable sum of money spent to make a success of it, it has been abandoned as impracticable, and they still continue to grind their materials by the wet process as of old.

Before proceeding further in my subject, it may be well for me to state my reason for bringing this subject up at all and explain the reason why I lay such stress on the matter of fine grinding. I recollect of no meeting held by our Society where this subject has not been brought up, and at several of them I have have had considerable to say, and when the subject was talked over in one of our discussions last year, I made the assertion that it is one of the most important enconomic problems in our business, as I hope to show before my paper is concluded.

It has been to me, till recently, an unsolved enigma, why English potters can fire a dense body, with a clear ring and a close fracture, at a temperature of Seger cone 5, whereas, our body at a temperature of cone 8 down and 9 begin-

ning to move, will not make so dense a ware, or so clean a fracture, while we have a body containing, as shown by chemical analysis, a percentage of alkali and fluxing materials, at least 35 per cent. greater than theirs. I don't wish to be misunderstood and have it supposed for an instant that I am comparing American and English ware to the detriment of the former, because the fact is now appreciated and has been for some time that we are getting just as good a price as they do in open competition.

What would it mean to us today, if we could bring our biscuit heat down to cone 5 instead of going to the excessive temperature we do?

First: A very important item, saving of fuel.

Second: Saving in saggars, on account of less heat employed, and the fact of being able to use very much cheaper clays than the very high priced ones we now have to buy in order to get sufficient refractoryness.

Third: The easier regulating and more even firing of the kilns at the lower temperatures.

This third reason is unquestionably a debatable one and one that I don't consider so important as the first two, as, from my point of view, there can be no possible question raised concerning these. I also claim that the finer grinding of our materials will not only help in reference to fuel and saggers and firing, but it will stop a large quantity of so-called mishaps that occur to us all without any apparent reason; mysteries that happen in all our potteries without any cause being discovered, and which disappear as mysteriously as they came.

Who is there here that has not run up against one of the following circumstances: A manager is called into the clay shops to see a quantity of ware cracked in the moulds, a very excessive amount, whereas the day before, and probably for months previous, the percentage of cracked ware is normal. He knows that he has got in no new material; all the clays, flint and feldspar have been taken from the same bins, and no error has been made in mixing. The next day, the trouble which existed practically with nearly all the clay, suddenly disappears and everything goes on as usual. What

has happened is probably this: The slip has been made too thin by the accidental running of too much water into the blunger. During the night the body has settled down in the bottom of the tanks into a hard mass, and if the slip pumps are started at the same time as the other machinery, nobody knows what is being pumped into the presses. It may be principally flint, or may be no flint, but whatever it is, it is not the body as originally mixed. The trouble does not end with your having some cracked ware in the clay shops. That which does not crack goes forward and fetches up in the glost warehouse as shivered or crazed.

It may be said that this is a very far-fetched case and not liable to happen, and if it did happen, can easily be remedied. In answer, let me ask: Is it remedied? Is it a common practice to start the agitators at least one hour before the pumps start?

Now the question is, will finer grinding help this difficulty? Most certainly it will, and the demonstrations I propose to make will convince the most skeptical.

I have here samples of two bodies made as under:

No. 1547—	American ground flint....	106
	Feldspar, regular ground..	42
	China clay..... ...	104
	Ball clay.....	59
No. 1548—	English ground flint.....	106
	Feldspar.....	42
	China clay....	104
	Ball clay.....	59

You will take particular notice of the fact that these two bodies were fired together in the same saggar and the series of cones 7, 8 and 9 were fired with them and that cone 7 is only slightly turned, and this is not a proper temperature at which sound biscuit earthenware can be made under our present system, yet you will find on comparison, that while body No. 1547 is very absorbent, No. 1548 is not nearly so much so. In fact, No. 1548 would pass as fair biscuit ware. Again, notice body No. 1547 is cut easily with the point of a knife, while No. 1548 will turn the edge of a knife, and lastly, as might be expected, there is a distinct differ-

ence in shrinkage between the two, No. 1548 being the smaller of the two.

These differences I ascribe to the fineness of grain to which the English flint has been ground. I had hoped to show in a very conclusive and positive manner by means of Schoene's elutriating apparatus exactly what this difference in grinding meant by making a series of determinations, and from their averages, give the quantity that passed over at the various current velocities indicated by the pietzometer readings.

Before giving you the results of my investigations by this instrument, I would like to state that it is comparatively new to me, and I hesitate somewhat in offering the determinations as evidence, because on the face of it I am in error, simply from the fact that I don't account for the 5 grains of flint with which I started in each washing test.

I will now give the result of my determinations:

	American Ground Flint.		English Ground Flint.
@ 1.25 Pietzometer,	.4576	.....	.7378
@ 5.10 "	.4378	.....	.4066
@ 17.50 "	.3027	.....	1.8878
@ 60.00 "	2.3558	.....	1.2034
Balance left, ...	.7298	.....	.3050
	4.2837		4.5408
Decantation.... ..	.1326	Decantation.....	.0889
Unaccounted for..	.5837	Unaccounted for,.	.3705
	5.0000		5.0000

If I interpret these figures correctly, the showing is in favor of the English flint, on account of the great differences in regard to the quantity passing over at the lower velocities. The American ground flint showed nearly twice as much passing over at the 60 cm. pressure as the English flint did. The difficulty I had to encounter in these investigations was this: That although I followed the best instructions I had as to the method of preparing the material, I was unable to get a clear liquid at the lowest reading, viz: 1.25 cm. In some of the trials I made, I allowed the mixture of the flint and water to stand two days, and after that time there

was a distinct milkiness, which no filter paper (at least in my hands) would arrest.

I mention this fact, which was identical in both varieties of flint, because I feel very sure that the items "Decantation" and "Unaccounted for" belong to the 1.25 cm. reading. I will further explain that the item "Decantation" was that portion of the liquid that still appeared as a cloudy fluid and *was arrested by a filter paper* and was weighed.

In short, if the figures of the test are rearranged as I think they really belong, we should have:

Description of Sediment.	English Flint. Percents.	American Flint. Percents.
Finest sizes, lost in process.....	7.41	11.67
Too fine to settle, but was caught on filter paper.....	1.78	2.65
At 1.25 cm. Pietzometer, slowest measured speed. Particles immensely fine.....	14.75	9.15
At 5.1 cm. Pietzometer; still exceedingly fine.....	8.13	8.76
At 17.50 cm. Pietzometer; still very fine	37.75	6.05
At 60 cm. Pietzometer. Particles relatively coarse, though still not to be distinguished clearly by the unaided eye.....	24.07	47.12
Coarsest particles, left after all others had floated away. Grains clearly visible to the eye..... ...	6.10	14.59
Totals.....	99.99	99.99
Total sediments down to and including the 17.50. cm. sediment.....	69.82	38.28
Total sediments, coarser than the 17.50 cm. sediment.....	30.17	61.71

In connection with the grinding of materials, I have so far only touched on the infusible portion, viz., the flint, but I now want to call your attention to quite as important a point, viz., the feldspar.

Some years ago, I made the experiment I have here repeated—and offer for inspection—but the marked difference in fusion as it is affected by the grinding is not nearly so

pronounced in this experiment as the one made sometime ago, because the experiment was not made under the same conditions.

The experiment consisted in grinding a batch of feldspar in a pan and removing from the pan at the end of each hour a portion of the feldspar slip. This slip was hardened on plaster and made up into cones, similar in size and shape to the Seger cones. These cones were then placed on a slab of white ware body, together with Seger cones Nos. 7, 8 and 9, and were fired in a regular biscuit burn. The feldspar taken direct from the bin gave a cone which stood up throughout the test without fusing. The cone representing one hour's grinding showed a distinct improvement in fusibility, and so on with each hour's grinding until the fourth, after which additional grinding seemed to produce but little effect. In the first experiment I made, I ground the trial on a pan having an area of 28 square feet. and this was made on a small color pan of 1¾ feet area, and on account of the difference between the weights of the runners in the respective pans, the degrees of fusion were not so clearly shown in one case as the other. Still, it will be seen that the sample marked "O," which is ordinary feldspar, taken from the bins, is very different from that marked 1, 2 and 3, but after it passes the fourth hour the change is apparently imperceptible, even up to the eight hours' grinding. On the rear in second row of cones on the trial slab you will notice cones 7, 8 and 9, cone 7 being down

Having stated my case, let us see whether anything practical can come from this paper. Is it of sufficient importance for this society to appoint a committee to devise a method for testing flint and feldspar? I refer, of course, to a mechanical test, which we might call the "American Ceramic Society's" test? Let us first of all know what we want, and then when we buy material, ask the seller if he will guarantee it to stand the society's test.

One half of all the earthenware we turn out is composed of flint and feldspar, and sometimes we hear of a certain firm of flint and feldspar grinders who grind very fine, but in nine cases out of ten the only question that is asked is will

it pass a 12*s* lawn well. Because it passes a 12*s* lawn, does not necessarily fit it for an earthenware body.

We may, if the Society desires to go into the question at all, make it a lawn test if that is the best, but, if they do, make it fine enough and say that a certain per cent. must pass the standard lawn they adopt.

To grind finer means more expense, but that need not prohibit its use. Suppose we do make a test, and the flint grinder says I will grind it as you wish but it will cost you so much more, then it is up to the user to say whether he wants it or not.

If any move is made in this direction, it must be made by the ceramic chemist, for it is eminently in his province.

DISCUSSION.

Mr. Stanley G. Burt: Mr. Mayer treats there a subject which is a hobby of mine. I have felt all along that the subject of grinding was of greatest importance; that the fineness of grain was the great keynote to many troubles, and many will remember that, as stated in my paper, that my results were equivalent to his. I did not go as far as he goes in making the suggestion of a committee to investigate the subject and set up a standard. I think the idea is very good; but in all my conversations with grinders, they say it is absolutely impossible to do that, because the additional cost necessary to get additional fineness of grind will not be stood by the American consumer. I could not give the figures, but flint furnished to pass a No. 14 lawn would cost considerable more than double if put into condition to pass a No. 16 lawn. It would mean considerable more than double the work and would add considerably to the cost. I think anything which might be accomplished in that line would be a great gain. While we may not be able to set up a standard, to say that it shall pass a certain lawn, we could insist on uniformity. I think it would be a good thing for the potters to agree among themselves upon a standard, say to pass a No. 14 lawn, then insist to the dealers that shipments must pass that lawn. I have found a number in my experience that would not begin to pass a No. 14 lawn. It seems to me

feasible to adopt a minimum standard, but I doubt if we could push it to a No. 16. But we could insist on the material passing a No. 14 lawn. I think this is one of the most important matters — fineness of grain — but I do not know whether there should be more committee work.

Mr. W. D. Gates: I move that a committee be appointed, of which Mr. Mayer shall be chairman, and Mr. Burt and Professor Orton other members, to take this matter in hand and make report.

I think we will always find that the buyer is the "doctor." If he insists on a thing, the people will "get busy" and get it, if his demands are anywhere within the range of possibility. We occasionally run across people ourselves who want things which we say at first are impossible, but if they insist on them, we have to go to work and try to make them. If fine grinding is done in England, we can certainly do it in this country.

Professor Orton, Jr.: I approve of the idea of this society trying to define something like a standard of fineness. I think it is an excellent idea. Whether we can enforce what we think is right and proper in the matter is another question altogether. I think what we do accomplish in that direction will be more in the way of giving each buyer arguments with which to hold up his end of the controversy, when it comes to dealing with the spar miller or flint miller, than in any other way. This society has no standing in the commercial world, and no lever by which it can bring into line either the spar or flint miller. Both are influenced by commercial considerations. It seems to me, therefore, that our attitude can be simply advisory. We set up the facts, but it be the buyer who enforces our findings with the spar and flint millers.

I am exceedingly pleased with the facts which Mr. Mayer has brought out. I have had a little experience with that Schœne apparatus myself, and if Mr. Mayer was here I could reassure him as to the quality of the results, which he feels so uncertain in presenting. The apparatus is not a perfect one, and I never yet saw a man who could take five grams of material, wash it into a number of grades of

fineness with the Schœne apparatus, and get back all with which he started. It seems physically impossible to save all the material; and I think Mr. Mayer has made, on the whole, a very creditable showing in his first attempt, and his results are similar to what I have myself secured and seen others get. He has proved his point by the use of this apparatus. There is no kind of question about that. The respective valuations of this one line (indicating on the chart) "2.3558 against 1.2034" show that. The American flint shows twice as much as the other. The whole experiment shows the great difference in physical characteristics. It seems to me that what a committee appointed by this society should do, is not to say what the spar miller should produce, or what the dealer should buy, but what the size of grain should be in order to accomplish certain results. Then let the commercial question settle itself.

For instance, Mr. Mayer says that beyond a certain limit there is no use for further grinding. Wouldn't it be well for this proposed committee to say how fine it is necessary to have material ground in order to produce certain results? Mr. Bleininger read a paper last year, touching on cement manufacture, in which he gave carefully prepared data on the sizes of grain of flint and spar. He illustrated the effect which the size of grain had on the decomposition of the material by lime, and clearly showed that either flint or spar above a certain size was well-nigh useless in cement, being simply inert material at the close of the process. But below a certain size it was chemically valuable, because, under standard conditions, it would be converted into soluble matter, and form a part of the cement. It was very clearly shown in that problem what the limiting conditions in cement work are. It seems to me, if we can do the same for the use of flint and spar and other insoluble fluxes in use in this country, and show that good results can be gotten from flint and spar of certain sizes, and recommend that they be considered as standard, then the whole situation will be cleared up, and anybody who desires to avail himself of our information or our work, can do so. It seems to me this is the only basis on which we can stand.

In regard to the constitution of the committee, I believe I was named as one, but I suggest that this would be a bad appointment, because I am not a great user of such materials and have little influence commercially. There are others here, who purchase thousands of dollars' worth a year, and other things being equal, they would have more influence.

Mr. Edward C. Stover: In looking over Dr. Seger's work on the question of grinding, find he touches that point, in giving the methods of changing the coefficient of expansion. In setting forth the ways of making the change, among other things, he says to grind the flint or spar finer. I took the matter up with a local spar grinder, and asked what the extra expense would be of making a finer grind. He said he had experimented along that line, and if I remember, he said that they ground an ordinary charge in a common ball mill, three or four hours longer, after the time when they considered it ready to deliver to the potter. He said in dry grinding, the difference in fineness of the material was so slight after double time, that to get such results as I wanted, would make the expense greatly more than any potter would pay. That came into my mind when Mr. Mayer's paper was read. I presume his American flint was dry ground, and the English flint wet ground. In short, he is comparing dry grinding with wet grinding, As I understand it, English material is delivered in the slop state, while here we get it dry.

Professor Charles F. Binns: I am very much struck with the calm way in which this work is handed over to a committee, without much idea of the immensity of the problems involved. It would probably take two or three investigators a year's time to standardize tests, in order to find out what they were really driving at.

The state of affairs in England is that the bulk of the larger producers grind their own material. Every large factory, I may say, without exception, has its own mill. The smaller ones buy the material ready ground in the slop state for the most part. This involves wet mixing instead of dry, which is not in accordance with the American idea of speed. The manufacturer cannot get out anything like as many tons in the same time by the wet process.

In England the materials are prepared and lawned in the slop state. The lawns are sixteen and eighteen for common wares, and each material is lawned before mixing. In some cases ball-clay is put through three decks of lawns. Under such conditions you would have to reconstruct every plant in the country and you will not find one potter in ten who would consider the matter even if you put to him the inducement that he can make better ware at a lower cost. You ask the Belleek manufacturer what is the secret of success in the manufacture of Belleek, and he will say that it is fine grinding. The material is ground until it is an impalpable powder; when tested between the thumb nails there is no grit. In a factory I was once connected with, there was a time when we kept a man all the time chipping spar, rejecting the brown parts of the spar. The question of grinding was raised and we made a test. A bucket full of the chips were put into the ball mill and ground perhaps six hours, and the Belleek made from it was every bit as good as that made from the selected spar, showing that the whole problem lay in the grinding.

I do not believe it is possible, with the present equipment of our flint mills, to give finer material. The cylinders are wood-lined, and the flint and spar are ground dry. If some miller had the courage to line his cylinders with porcelain or "French burr," and to grind wet, filter-pressing the slip and drying the product to within 5 per cent. of absolute dryness, he would have the pick of the market. Wet ground flint can be lawned, dry ground cannot, and this alone is a strong argument in favor of the wet method.

But, as a society, in the face of the fact that all the millers are grinding dry and all the manufacturers are mixing dry, we cannot, I fear, recommend any such standard as that which obtains in England. Unless we give the committee some definite instructions or prepare the way by preliminary work, it will not do much good.

Mr. F. W. Walker: The remarks of Prof. Binns are very much to the point and I think all of us, possibly, are familiar with these conditions. The grinders of flint and spar will not voluntarily grind finer. I had quite a talk with

one of them a few weeks ago, and he said they could not do it. I had just been talking with Mr. Mayer, and he said that to grind as fine as Mr. Mayer wanted it, would make the material cost double the money. He said, "Would you pay it?" I was not prepared to say whether I would or not. I was simply advocating along the lines of Mr. Mayer's paper. I think the subject wants to be thoroughly considered, but I do not know any one better able to consider it, and make report, than the committee suggested.

SUBSEQUENT COMMUNICATION FROM MR. ERNEST MAYER.

Although not present at the meeting, I am very glad to have the chance of seeing the stenographer's notes of the account of the discussion on my paper.

I fully expected, in fact anticipated, the criticism made by several that the grinders of flint and feldspar would protest against any change in the present method of grinding. My answer to that is this: That as a matter of fact the grinders of flint and feldspar don't know how coarse or fine they are grinding, and my experience is that they grind material as short a time as they possibly can, and the only limit they admit is when the buyer gets a car load of material that won't go through his lawns and won't pay for it. Then, and only then, will they grind finer, unless it so happens (as has many times been done) the potter will charge his lawn to suit the coarser flint.

My paper was written as a protest against the haphazard method of the grinding now in vogue, and, while the point was omitted in my paper, I fully intended to bring out the fact of the great irregularity in grinding in the different cars delivered.

This whole matter is a very simple business proposition and resolves itself into the following: The flint grinder has a grade of flint to sell at a certain price which is ground a certain number of hours, which we will call his *regular flint.* Now for every hour's grinding after that point a fixed sum should be charged, and it will then be up to the manufacturer to decide what he wants.

A committee appointed by this association could ascertain exactly what the finer grinding meant by making pieces of ware from flint and feldspar ground in the different number of hours, and if the results warranted it, after the results of the experiments had been laid before the association, each member would would take advantage of the information gained and govern himself accordingly.

I am not foolish enough to think that my paper will be the cause of revolutionizing the flint and feldspar grinding business, but if the remarks I have made carry any weight, by the arguments used to substantiate them, then my paper has accomplished its mission, viz.: that finer ground material than we now get is one of the means of making more reliable and better ware.

There is one matter to which I would like to call the association's attention, and that is, what is a No. 12*s* lawn? As a matter of fact we come to these meetings and talking about glaze or body being sifted through a No. 12 or No. 14 lawn, it appears to me that no two of us are talking about the same thing. As an example: Professor Binns in discussing my paper says, "In England the materials are prepared and lawned in the slop state, the lawns are 16 and 18 for common ware." Now, evidently, we are not talking about the same thing, because if the common ware goes through an 18*s* lawn, what kind of a lawn do they use for the best ware? Except for stain, I never heard of a lawn being used either here or in England finer than 16.

My understanding of a 12*s* lawn is that it means 30 meshes to the ¼ inch, or 120 meshes to the inch; a 14 lawn 35 meshes to the ¼ inch = 140 meshes to the inch, and a 16*s* lawn = 40 meshes to the ¼ inch = 160 meshes to the inch; but I think the question of the names and number of meshes to the inch ought to be settled and to arrive at an understanding of what the various sizes actually mean in count.

CRYSTALLIZED GLAZES.

BY

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During the first three years of the existence of this Society we have added quite a good many valuable contributions to the knowledge of silicates. Silicic acid in its various forms has occupied our attention more than anything else, and all papers tend to demonstrate the importance of fineness and most complete distribution. When older methods stopped, after showing that a certain amount of silicate matter was present, we went one step further in trying to decide, if it was qualitatively suitable to the purpose as well as quantitatively. Our transactions contain most important researches in regard to tensile strength of clay-mixtures, plasticity and the relation between plastic clays and the non-plastic ingredients of ceramic bodies. Glazes and fluxes have had their share in our discussions, and even the origin of kaolin, and the nomenclature of the silicates has been touched upon, not to mention all the papers treating on the subject of firing kilns, and similar topics, in which the manufacturer of brick is much interested as the manufacturer of fine china.

Yet, to be frank, there seems to be a large deficiency in the knowledge of our ceramic bodies themselves, and I almost believe that the slow progress in establishing uniform rules and tests in ceramics is partly caused by insufficient knowledge of the very foundation of our ceramic art. What are our bodies and glazes? In some of them, baked at a very low temperature, the various components have hardly entered into a chemical action and maintain as finished ware more or less the nature of a mechanical mixture. Others become quite uniform, and seem to undergo such an intense change, that the final product appears to be almost a chemical compound. However, the chemists claim that a chemical

compound cannot be still considered as a union of various molecules; but where can we draw the line between a chemical compound and a union of various molecules in our ceramic products? May we consider the translucent hard porcelain as a well defined compound, or is it not rather still a union of molecules, of substances partly dissolved, partly suspended, in the melted feldspar? As Prof. Orton said in one of his previous papers,* we have been working for years with test tubes and balance, seeking to explain the marvelous differences and similitudes, but we have decidedly neglected the use of the microscope, and I would like to urge the members of our society to become microscopists and to open up new methods, which will not only allow us quick investigation, but will very likely lead to a more thorough knowledge of ceramic bodies and materials. Such methods might also give us a chance to discover in our clays and kaolins a variety of the rarest elements, which we could not detect with our slow chemical analysis, but which undoubtedly will have certain influences.

There might be a way of crystallographic analysis. We all know, that in chemistry we crystalize a substance repeatedly in order to purify it. The process of crystallization is of the utmost importance, and we shall advance more rapidly in ceramic theory, if we devote more attention to crystals and crystallization. In kaolin, as well as generally in ceramic bodies, the crystals are very fine grained, but of late glazes with splendid crystallizations have occupied the attention of the ceramic world, and although those articles with crystallized glazes are too difficult to be manufactured on a large scale and too high priced to become a staple article, they are very interesting from a theoretical point of view. They are, at least, a start in a new direction and I beg permission to occupy some of your time in describing the methods of producing them.

Our glazes are more or less perfect glasses, in very thin layers and may be considered as solutions of silicates, which although not uncrystallizable, do not flash readily into the crystalline state. However, those glassy silicates form

*Trans. A. C. S., Vol. II, page 100.

definite crystalline compounds, if given a long time to cool. Actually, the crystalline or glassy conditions are more or less accidents of cooling. Crystalline segregations are not new in colored glazes. Aventurine effects are found on Japanese vases made centuries ago, and I have now in my possession a Japanese vase, showing tiger-eye effects on some parts, which was certainly not made within the last twenty years, even if I do not venture to determine its true age.

Dr. Maeckler, by request of Director Dr. Heinecke, of the Royal factory at Berlin, who had purchased some Rookwood vases at the World's Fair, 1893, for the museum of the factory, made the first scientific investigation of the phenomenon, which was published in 1896. Aventurine glazes have been made by various factories in the United States and in Europe. I recently received a nice collection from Egersund, Norway, but I do not know of any one who has reached the perfection of the crystalline glazes of the Rookwood pottery. However, the technique of crystallized glazes was not further developed, until at exhibitions at Paris and Brussels and also at Stockholm in 1897, the French National factory of Sevres, the Royal factory at Copenhagen, and Rohrstrands Aktiebolag, of Stockholm, came out, almost at the same time, with artistic vases covered with crystallized glazes, while the firm of A. Bigaud & Co., in Paris, was successful in the same direction on stoneware. In 1898 the Royal manufactory, of Berlin, followed with some splendid specimens, and of late quite a few factories have been successful in producing crystallized glazes, and although these art goods cannot be produced by the thousands, the time has passed when the formation of crystalline glazes could be looked upon as a mere accident.

It seems as if the very first study on that subject was made by Ebelmen as early as 1847 – 1852, but the credit as inventors of those crystallized glazes is due to Charles Lauth and G. Dutailly, who furnished a cup and paper on the subject for the museum of the National factory at Sevres on September 19th, 1885. An interesting article, "Sur les Couverts de Porcelaine," by Chas. Lauth and

G. Dutailly, is to be found in the book: "La manufacture nationale de Sevres, 1879–1887," par Charles Lauth, in which they say:

By introducing oxide of zinc into glazes, which increases only slightly the fusibility of the glaze, it is possible to obtain highly basic glazes, which form during the process of cooling small crystals of silicates or more likely of oxide. The best crystals were obtained in a glaze of the following mixture, melting at 1350 deg. Cels.

Pegmatite	55.60%
Sand	16.00
Kaolin	4.40
Lime	12.00
Zinc Oxide	18.00
	106.00

This would correspond to the following composition of the glaze:

SiO_2	57.49%
Al_2O_3	11.68
CaO	6.72
Na KO	6.12
ZnO	18.00
	100.01

In another short paper, read before the International Congress for Applied Chemistry at Vienna, 1898, Clement of Copenhagen describes his experiments with the silicate of zinc on the hard porcelain of the Royal factory, Copenhagen. On the porcelain, which was first heavily glazed with the regular glaze and fired as usual, he applied with a brush a second heavy coat of $SiO_2 + ZnO$. After the second glost fire the glaze appeared to be saturated with small crystals, which he claims to be crystals of the natural silicate zinc, Willemite, Zn_2SiO_4. In his glazes, which must be burnt in oxidizing fire, the coloring oxides of metals gives the following effects: Oxide of copper celadon-green, oxide of cobalt, blue, chromium and tin together a yellow-red and the oxide of nickel, which would generally cause a brown color, gives in combination with the silicate of zinc a Persian blue glaze with blue crystals.

A more recent publication of G. Vogt, technical manager of the National factory at Sevres, says that the first crystallized glazes were made at Sevres in 1884. Since then the process has been greatly improved and is applied with good chances of success to stoneware as well as to porcelain. It has been found of great advantage to fritt all materials for the second or crystallizing glaze-coat in a strictly oxidizing fire, for which fritting crucibles are used. He gives the composition of two fritts in practical use at Sevres as follows:

	I	II
Pearl ash (K_2CO_3)	138	69
Zinc Oxide	162	202½
Flint	360	350

Both fritts were mixed; best result was obtained by mixing 35 parts No. I and 15 parts No. II.

A yellow crystallized glaze was obtained by fritting

	III
Pearl ashes	138
Zinc Oxide	162
Flint	300
Rutile	82

15 parts fritt No. II and 85 parts fritt No. III are mixed to good advantage. Rutile contains a good many impurities, principally ferric oxide, and this seems to be the coloring agent, while chemically pure titanic acid will give pretty colorless crystals.

Summing up all these publications, there seemed to be no doubt, but that the silicate of zinc was the essential substance, which, being dissolved in the melted, liquid glaze, would separate again in slow cooling, and I concluded by following this general idea, it would be impossible not to obtain crystals. I proceeded, therefore, to fritt in equivalent proportions pure zinc oxide and silica, using a 99 per cent. dry ground flint. Then I prepared a thick slip by mixing the finely ground frit with water and applied a heavy coat on various small articles of our Wheeling pottery fancy goods. They had been through the glost-kiln before, and for this second glost-fire, in order to obtain crystals, I placed them in a covered pot, put the latter into a large sagger, which I filled solidly with sand, placed the whole in the

middle of the second ring, well protected from any possible draft after the large kiln would be open, and felt satisfied that I had done everything in my power to procure a very slow cooling off. But the result was anything but gratifying. The very best that I could show for all the trouble were some white formations like moss, which proved to be something like small crystals under the magnifying glass, but which were quite different from the splendid specimens I had read about.

I had no chance to investigate any further, but of late my observations were confirmed by the experiments of an Austrian ceramist, who published his results under the anonyme "Bries" in No. 2 of the Sprechsaal, 1902.

He started from a china glaze of the formula

$$\left.\begin{matrix}0.4\ K_2O\\0.6\ CaO\end{matrix}\right\}\ 0.5\ Al_2O_3\quad 4.5\ SiO_2$$

and increased the basic side by adding one equivalent ZnO.

The new glaze which had to be fritted, would correspond to the formula

$$\left.\begin{matrix}0.2\ K_2O\\0.3\ CaO\\0.5\ ZnO\end{matrix}\right\}\ 0.25\ Al_2O_3\quad 2.25\ SiO_2$$

The body he was experimenting with had the following composition:

Clay substance	35%
Flint	40
Spar	25

The trials were fired at a temperature of Seger cone 8 that is at a heat quite frequently reached in our American biscuit-kilns. But neither the glaze mentioned above, nor a plain potash zinc bisilicate

$$\left.\begin{matrix}0.3\ K_2O\\0.7\ ZnO\end{matrix}\right\}\ 2\ SiO_2$$

gave the desired result. Then he continued: "I did not succeed in obtaining gratifying results as long as I restricted my experiments to glazes without titanic acid. There was only a tendency to small formations, which sometimes could be proved to be crystals under the microscope. In consequence I went one step further and fritted another glaze, in

which part of the silicic acid was replaced by an equivalent amount of titanic acid,

$$\left.\begin{array}{l}0.8\ K_2O\\0.7\ ZnO\end{array}\right\}\quad\left\{\begin{array}{l}1.7\ SiO_2\\0.3\ TiO_2\end{array}\right.$$

obtained by the following mixtures:

Pearl Ash	18.43%
Zinc Oxide	25.24
Flint	45.39
Rutile	10.94

"The glaze became opaque. Titanic acid does not go into a complete solution in the liquid glaze, its molecules remain more or less suspended and this circumstance seems to be very material for the formation of large crystals. Besides a hard biscuit, a long period of an evenly high temperature at the end of the burning and, as mentioned before, a slow cooling are the most favorable conditions." He also recommends to burn the crystallizing glazes in double saggers and calls attention to the necessity to have the inside of the sagger well glazed, as a porous sagger would absorb alkalies from the glaze; which might be detrimental to the crystals. Another interesting item is the fact, that in colored crystallizing glazes the crystals sometimes have taken up the coloring oxides and appear in their color, while the ground shows a different shading.

The effects obtained by this application of titanic acid are really wonderful and I exceedingly regret, that I cannot send along some of the fine specimens standing before me while writing.

DISCUSSION.

Mr. Hasburg: I have a small specimen of the Copenhagen crystalline glaze with me which I will pass around if the members care to see what this paper refers to.

Mr. W. D. Gates: One thing which has struck me is the use of the microscope in the study of crystalline glazes. I was much interested in having it pointed out to me that we we manufacturing a matt glaze which was a mass of crystals. Until Mr. Burt called my attention to it, I did not know what we were making.

Mr. Ray T. Stull: I was interested in Dr. Zimmer's paper, as I happened recently to find in Sprechsaal the same article which he speaks of, about crystalline glazes. I immediately became very much interested. I weighed up a small batch of glaze and fritted it, and applied it to a hard porcelain body which burned very dense at cone 8. The factors which go to produce a crystalline effect were given in this article as, first, a continuously oxidizing fire; second, a gradually increasing temperature; third, a gradually decreasing temperature; and fourth, the elimination of the alumina. The composition of this glaze was

$$\left.\begin{array}{l}0.3\ K_2O\\0.7\ ZnO\end{array}\right\}\begin{array}{l}1.7\ SiO_2\\0.3\ TiO_2\end{array}$$

This glaze is free from alumina, and when fused, lies quietly. I think the paper said the titantic acid did not go into solution. In the fritt I made, I found it did. After making the fritt, I applied the same in a dab in the center of a small tile. I did not have access to a kiln having the proper temperature at that time, and so I put it back into the fritt kiln, where it is almost impossible to maintain oxidizing conditions, or to raise the temperature slowly, or to cool slowly. However, I did the best I could with these unfavorable conditions. On examining the results, I found the center was perfectly clear, but around the rim of the glaze were these little flossy, silky crystals, which we saw in the piece which Mr. Hasburg passed around.

Professor Binns: In regard to the production of crystalline glazes, it seems to me that it must always rank with the Rookwood Tiger Eye or Gold Stone, as more or less of a ceramic curiosity. It must not be supposed that it will be of commercial value. It ranks with the fine art productions which made the old Chinese so famous. Such productions are fine art and not science. I am coming more and more to the conclusion that the production of fine ceramics is as much a matter of art as of science. This is illustrated by a conversation which I had with a Japanese student in my school regarding the production of copper reds. He says that the most successful man in Japan in the production of copper reds told him that about one piece out of two hundred is

successful, and moreover he finds it a mistake to use artificial material if he can get the natural. He won't use ground silica in any form. He gathers together a mass of rice husks and calcines them to procure finely divided silica from the ash. This opens the limitless field of research (if it was limited before, which I doubt). The Japanese potter who picks up a stone by the roadside and uses it obtains a result which he cannot duplicate, but he may get a fine thing. The production of crystalline effects belongs to the same class of beautiful and rare ceramics, of which we are beginning to take cognizance. We are coming to see that there is more than cold science to be reckoned with. It is the art which make a physician a good physician, and the art which makes a potter a good potter. It is the art which has made some practical men superior to some men depending only on science, and which we always have to take into account in the production of finer work.

Mr. W. D. Gates: Regarding that Japanese silica derived from rice husks: doesn't that produce an intensely fine silica?

Professor Binns: That was the point in glazes. This intimate mixture of silica and carbon, which they depend on to help the reduction of copper to the red color, was secured from rice husks.

Mr. J. W. Hasburg: Concerning copper reds being so accidental as one in two hundred,—that might be the case with the man who did not know his business and does not know how to get the copper red. I do believe it is a fact from a scientific standpoint, that it is a purely chemical process. It is produced by special conditions and chemicals, and when the conditions and chemicals are right, the red will be produced. I believe that where the production of copper red is thoroughly understood, it is not a case of one chance in two hundred, but that it will be produced nearly every time with little variation. I have succeeded in producing these reds and, therefore, can speak from experience.

Professor Binns: Just as I said before—the production of copper red is not under the control, absolutely, of any known set of conditions. Of course there are special conditions to produce it, but at the present time we do not know

exactly what they are. We could hardly find a higher exponent of science than Seger; yet his copper red is not to be compared with that produced by the famous Lang Yao family in China.

Mr. W. D. Gates: My experience with copper red is that you want to get your theories in good shape, go to work and flirt with it — then do something else.

Professor Binns: It is a matter of common knowledge that one of the Havilands of Limoges spent 150,000 francs in an effort to produce copper reds, and after some years consumed in experimenting, gave it up as a failure. Looking at some of his samples in the light of present knowledge, I think I can see where he failed, but I don't believe I could do any better.

STONEWARE SLIPS OR ENGOBES.

BY

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Engobes or slips are natural clays or clay mixtures designed to cover other clay bodies, whose color we desire to conceal. They also can serve as ground-layers for decorative effects, in case the article is to be decorated. Generally an engobe is used to increase the value of the product on which it is applied. In these cases, the engobe is a more expensive mixture than the clay body on which it is used. Sometimes its value is the same as the clay on which it is applied, and only serves to cover the color.

The use of engobes is a very old one, and history tells us that the Greeks already used engobes in three colors—white, red and yellow. Edwin A. Barber, in his book, "Pottery and Porcelain of the United States," says in regard to engobes: "Perhaps there are no products of the potter's art more interesting to the antiquary and the collector than the rude 'slip-decorated' pieces which were made in England and Germany during the seventeenth and eighteenth centuries."

Engobed ware was made in Pennsylvania as early as 1762 and the industry flourished until about 1850. Engobed ware was not made thereafter in large quantities until the art potters took it up some years ago. Stoneware manufacturers are now using it more and more, and we find quite a nice variety of "white lined" clay products throughout the country.

In the art branch of ceramics, engobes are extensively used. These engobes are compounded in order to obtain decorative effects and are commonly termed "colored slips." Terra-cotta manufacturers are using engobes to a very large degree, and produce some very good effects with the same.

The nature of engobes can be twofold. They can be white or colored. In the first instance, they are mixtures of

pure white burning clay materials, and in the second way, they can be natural colored clays or mixtures of clay, tinted with color-producing mineral ingredients.

On stoneware, the engobe is usually white, while on artware and terra-cotta, mostly colored.

Engobes are mostly applied to the ware in the green state, either by dipping or spraying. Experiments have been made to apply the engobe on the dry ware, with various success. It is evident that both ways can be used, and it depends upon the product on which the engobe is to be applied.

Engobes can be covered with a glaze, or they may themselves form a vitrified coating. When an engobe is covered with a glaze, it can be applied to the engobe in green state, or after the engobe is burnt once. To the first class belong stoneware, and to the other class, artware.

In Germany we find a few clays which are put on the market and are specially adapted for engobe purposes. We do not have any special grade of this kind of clay on the market in this country, as clay-miners never have made any effort to foster that trade.

In order to make a clay valuable for white engobe purposes, it should possess a certain degree of plasticity; it should burn white; it should be able to adhere to another clay in such a way as to give a uniform coat when applied as a thin slip; it should have the same shrinkage as the clay on which it is applied, and should be able to hold a glaze without defects.

According to Dr. Seger, the two German engobe clays which are on the market have the following composition:

	Kaschkau Clay	Cologne—Meissen Clay
SiO_2	63.17	62.52
Al_2O_3	25.09	25.57
Fe_2O_3	0.64	0.92
CaO	0.35	0.65
MgO	0.26	0.10
K_2O	0.80	1.04
H_2O	9.70	9.27
	100.01	100.07

Rational analysis of both:

Clay substance.	67.82	72.05
Feldspar.....	1.25	0.10
Flint.....	30.93	27.76

I have so far been unable to find in America a white burning plastic clay which would answer for engobe purposes, and which would have nearly the same composition as the German Clays.

The clay mined and marketed by Fischer, Black & Co., of Newport, Pa., is the only clay of which I know which comes close to the German engobe clays cited above. The analysis is given below:

	Analysed by Langenbeck	Analysed by Geijsbeek
SiO_2.	62.89	62.30
Al_2O_3	26.14	27.07
Fe_2O_3	0.57	0.63
CaO	0.60	0.64
MgO.....	0.75	0.13
K_2O	2.32	1.85
H_2O.....	7.55	7.74
	100.82	100.36

Rational analysis:

Clay substance.	70.81	68.70
Feldspar.....	2.17	2.42
Flint.....	27.84	29.24

I have made some thirty different experiments with this clay for engobe purposes without any satisfactory results, and have come to the conclusions that this clay does not answer for that purpose, although it is in chemical composition nearly like the German clays.

In making further experiments along this line, I used various American kaolins as they are put on the market, and of the numerous varieties tested, the Florida kaolin and the Graham kaolin answered the purpose best. I first used the Spencer kaolin, with various success, but abandoned the same later on for commercial reasons.

In conducting such experiments, there are at present no rules which could be used in guiding one in order to obtain satisfactory results, and I am sorry to state that so far, my experiments have not yet given me a basis from which to formulate a rule which would be useful in making further experiments.

It is, strictly speaking, an experiment in which plasticity and shrinkage play the greatest part.

I have found that in making colored engobes it is much easier to obtain a satisfactory clay than in making white engobes. For such purposes I have been able to use regularly the ball clays without any additions. As coloring material, I have found that the ordinary underglaze colors on the market answer the purpose very well.

In making engobes for stoneware, it was always desired to have a white engobe, which could be used for inside work, such as for jars, milkpans, buttercrocks, churns, and so on. Stoneware people call these engobes under the familiar name of "white linings or enamels."

My first experiments were conducted on washed stoneware clay as used by Sherwood Bros., New Brighton, Pa.

The rational analysis of this clay is:

Clay substance	66.54
Feldspar	2.46
Flint	31 02

I determined first the relative shrinkage between the Florida kaolin and the stoneware clay, as I intended to use the Florida kaolin as a basis for the experiment.

A thin coat of kaolin slip on the stoneware would be shivered off entirely, after burning, showing that the kaolin shrunk more than the stoneware clay.

The addition of non-plastic material was next tried. 75 per cent. of kaolin, and alternately 25 per cent. of flint, feldspar and whiting. Of these three experiments, the two last showed the best results, and further experiments were made in that direction.

Engobe Number	B_2	B_3	B_4	B_5	B_6	B_7	B_8	B_{19}	B_{21}	B_{20}	B_{22}	B_{24}
Florida kaolin	75	75	75	75	75	75	75	70	70	65	65	65
Brandywine Feldspar	25	..	20	15	12.5	10	5	10	15	10	25	30
Whiting	..	25	5	10	12.5	15	20	20	15	25	10	5

Of the above twelve experiments it was found that B_7 answered best for stoneware slip, and next best was B_{24}. The difference between the two was that B_7 was whiter than B_{24}. None of these mixtures peeled on the stoneware clay, the only defects they showed was cracking in drying on a few pieces. The only trouble with B_7 was that the shrinkage was just a little less than the body, so that when applied on the outside of the ware it would crack off the edges of the body. Therefore it was advisable to see what another kaolin would do. Spencer kaolin was substituted for the Florida kaolin. This substitution obviated the trouble above mentioned at once, and the mixture gave very satisfactory results,

While engobe B_7 did not shrink enough, and none of the other experiments showed any improvement in that respect for outside work, I concluded that the cause was due to too much plastic clay. I calcined some of the Florida kaolin, and made mixtures with the same formulæ B_7, but varying proportion of calcined kaolin. I took account of the loss of weight in the calcined kaolin, in weighing it into the new test batches, and obtained an excellent engobe for outside work. B_{29} gave the best results.

Engobe Number	B_{27}	B_{28}	B_{29}
Florida Kaolin, Raw	60	50	35
Florida Kaolin, Calcined	15	21.5	34.5
Brandywine Feldspar	10	10	10
Commercial Whiting	15	15	15

These few experiments show that there are several ways to get a suitable "white lining" or engobe for stoneware, and judging from past experience with the above engobes, I think that B_{29}, where calcined clay was used, would give in the long run the best results, especially on stoneware, where

body, engobe and glaze are all brought to perfection in one burn.

All samples were fired at cone 8, and the glaze employed for all engobes was

$$\left.\begin{matrix}0.3\ K_2O\\0.7\ CaO\end{matrix}\right\}\ 0.5\ Al_2O_3\ \ 4SiO_2$$

In connection with these experiments, I beg to call your attention to the solubility of bodies and engobes in glazes. When I started these experiments, I was told to use the regular white stoneware glaze employed by Sherwood Bros. at that time. I judge it was a zinc-boro-lime glaze, but do not know its composition. On using this glaze, all engobes showed that the layer of white material was attacked to such an extent, that the body was plainly visible, and the effect of the engobe was lost.

Engobe glazes should be so compounded that their solvent power is very low. Easily fusible glazes, which do not need a high fire to be called "finished," will not dissolve any important part of the body, if they are not fired higher than their proper finishing point. But when fired at a high heat, their solvent action on saturating themselves with the ingredients of the body, is very great, and such glazes can not be used for engobe purposes. Glazes, as generally used in stoneware potteries, high in alumina and silicic acid are usually well adapted for engobe purposes.

Some time ago I was called upon to make engobes suitable for western stoneware clays. I soon found that engobes which work well on one clay, may not work so well on another.

The clays in questions were the Colchester, Ill., and Tennessee, Ills., stoneware clays as used in Macomb and Monmouth, Ills.

The rational analysis of these clays is as follows:

	Colchester	Tennessee
Clay substance ...	54.60	65.70
Feldspar.....	4.51	2.25
Flint.....	40.89	32.05

The Tennessee clay resembles very closely the New Brighton clay in rational analysis, and works about as well.

Engobes B_7 and B_{24} did not work well on these clays. B_7 would crack and lift up in drying, and peel afterwards, while B_{14} would only crack.

A substitution of Florida kaolin for Graham kaolin was made. It was found that Graham kaolin No. 1 was a better product than Spencer kaolin. It shows the same properties as Spencer kaolin, and has nearly exactly the same shrinkage. In rational analysis they are nearly alike.

	Graham Kaolin No. 1	Spencer Kaolin
Clay substance....	95.88	94.12
Feldspar.....	0.12	1.50
Flint.....	4.00	4.68

The results of the substitution, B_{51}, was an improvement over B_7. The only defect was that when the glaze was applied, the engobe would blister. After some experiments it was found that a combination of engobe B_{51} and B_{24} would give good results.

Engobe B_{56} which was adopted as satisfactory for both the Colchester and Tennessee clays had the following composition:

Florida kaolin..... ...	32.5
Graham kaolin.....	37.5
Brandywine feldspar.....	20
Commercial whiting.... ..	10

This engobe worked well on dry and green body; it could be dipped in and outside at the same time; and the burning was all done in one fire.

As it seemed strange that flint could not be used so far in engobe, I have started experiments on that subject, but as yet I have been unable to finish the same or come to any conclusion.

As stoneware glazes have a tendency to pin-hole, it often happens that the engobe underneath will be discolored in use, by absorption of dirty liquids through the glaze, and with a view to obviate this trouble, I have started another set of experiments which will give vitrified engobes on stoneware bodies. The results so far have been very satisfactory, and I have produced two engobes which get around this trouble.

One experiment was started on the line of vitrifed bodies, and results were obtained with a formula of the following composition:

$$1\ K_2O \quad 2.15\ Al_2O_3 \quad 10.80\ SiO_2$$

and the other experiment was started on the stoneware glaze line. It had the formula

$$\left.\begin{matrix}0.50\ CaO\\0.50\ K_2O\end{matrix}\right\}\ 1.00\ Al_2O_3 \quad 4\ SiO_2$$

All experiments were glazed with the stoneware glaze as mentioned before and burnt between cone 6 and cone 8 heat.

DISCUSSION.

Mr. W. J. J. Bowman: I do not know that I have anything to add to that paper. My experiments in a certain way have followed much the same lines as those outlined in the paper, and his experience has been mine. I would like to say that I think it is necessary to have a proper body underneath, as well as a proper slip. In other words, I think there are certain bodies to which we cannot make a slip stick. At least, that has been my experience.

I would like to raise one point, and that is whether the author means that the slip comes off in firing or whether it comes off when applying it, or drying? He mentions in one place that the slip peels off the outside. In some cases, I find it will do this, if the shrinkage of the slip is not sufficient in the green state; in other cases, it will stay on in the green state but come off when fired, which is due to quite another condition. One fact which I have experienced and never could understand is this: I have had a case where the shrinkage of two different bodies would be entirely different and yet the same slip would apparently work alike on either. The shrinkage of the body did not seem to take much effect on the slip. It would shrink down with one body, which shrank twice as much as the other.

Professor Orton: Do you mean the shrinkage occurring in firing or in drying?

Mr. Bowman: In firing. One would shrink an inch to the foot and the other an inch and a half, and yet the same slip would apparently work as well on one as the other.

Mr. Stanley G. Burt: The slip, however, would have a still greater contraction than the body in both cases.

Mr. Bowman: Yes.

Mr. E. C. Stover: Is not the explanation to that, that the deficiency, as it were, of certain ingredients in the mixture is supplied by an excess of it in the body? I think the line of experiments made by Mr. Binns seemed to show that. Under a very hard fire, the mixture of engobe being deficient in one of the ingredients, and there being an excess of that in the body, the hard fire will make it borrow, as it were, from the body and therefore change its shrinkage and make it fit the body.

Mr. Burt: I think the slip undoubtedly undergoes a chemical change, much as the glaze does. When you get a slip which has an excess of contraction, the moment you have vitrification of the slip, there is an incipient action on the body; so if the the slip vitrifies, it has entered into a certain chemical combination with the body. As long as you have a porous engobe, there is a mere mechanical bond which is easily broken; but as you approach vitrification of your slip, you get a chemical bond which is more difficult to break.

Mr. Bowman: The question I raised was how the slip adjusted itself to the different shrinkages and did not tear.

Mr. Burt: It has a certain elasticity, you know. If you should go beyond that limit of elasticity of the slip, it would tear.

Professor Binns: I think the probable explanation is that the slip in the first place was adjusted to the larger body. If it had been adjusted to the smaller shrinkage, it would scarcely have accommodated itself to the larger. The extra shrinkage could be taken up by the body, but if it had been adapted to a smaller one in the first place, it would peel on the larger. I will ask Mr. Bowman if he did not find, after all, that the cohesion of the slip and the body was less important than the cohesion of the glaze and the body—providing the upper and lower agreed, the intermediate layer did not so much matter? If the glaze agrees with the body, the slip can have a wider range. And if you have a glaze

which will shiver on the main body, even if you apply a good slip to it, it will shiver, the slip peeling right off with it.

Mr. Ray T. Stull: I happened accidentally to produce something in a recent experiment which may throw light on the subject. I made a China body and casted two small vases from it, using the same mould. One I packed in flint, inside and out, and burned to cone eight. The other I did not pack in flint, and burned to cone four. To my great surprise, the one burned to cone eight was larger than the one burned to cone four. The flint on the inside of the one finished at cone eight prevented shrinkage, while the one not packed in flint was perfectly free to shrink.

Professor Orton: I am glad Mr. Stull rose to make that point, as I was about to ask him to do so. It seems to me a very pretty little point with a clear application in this connection. He showed me these trials, and I was surprised to find such difference of size in two vases, cast in the same mould, of the same slip. Both were vitrified, both thoroughly hard, both translucent. The quality of the translucency in the one which shrunk the least, was somewhat deficient as compared with the other; but this may have been due to the fact that the flint packed inside of it had adhered slightly to the inner surface, and did not come off perfectly clean, and thus made a slight lack of translucency. You can see the shadow of your finger through the walls of the vase in both cases. The one which had a mechanical bar to shrinkage, underwent vitrification without condensation, and the one which had no bar to shrinkage, underwent vitrification and condensation as well. If this would occur in the case of Mr. Stull's porcelain body, I do not see why it would not work in the case of a stoneware slip.

Mr. D. C. Thomas: Which of the two vases shrunk the most ?

Mr. Stull: The one finished at cone four, which had no resistance to shrinkage. I might add to that, that the difference in shrinkage between the two was at least three per cent. The one burned to cone four shrunk at least three per cent. more than the one burned to cone eight—I presume more.

Mr. Burt: I would like to ask Professor Orton how that bears on the question of Mr. Bowman's.

Professor Orton: We have here a case where a porcelain vase, not dissimilar in composition to the stoneware slip, had a mechanical bar to shrinkage in the shape of a whole lot of flint packed inside of it. That vase was carried up to the vitrification temperature, where ordinarily it would shrink greatly. The little vase which was not packed with flint had an opportunity to shrink and did not shrink. The other one which could not shrink, went on and vitrified all right, and became translucent, showing that the chemical reactions took place in their usual order, but that the vitrified walls of the vase were sufficiently viscous to take the size of the mass of flint which they held inside.

Now, Mr. Bowman says that he put a slip, which we will assume is similar to a porcelain body—(I don't know how closely similar, but of the same general class of substance)— he put this slip on two bodies. One body shrinks a good deal and the other less. It stays on both without rupture, and he asks why. It seems to me that the cases are parallel. There is a mechanical bar to shrinkage in both cases. In one case it was the flint with which the little vase was filled, and in the other case it is the comparatively non-shrinking body upon which the slip is fastened.

Mr. Burt: That would necessarily assume that the high shrinkage did not take place until after vitrification had taken place; because, as I said before, there is nothing but a mechanical bond between the clay and the slip and before vitrification any tension of any extent would very easily rupture that bond. So if we are to assume that vitrification of the slip enabling it to hold to this body is due to the fact of a bond in any way equivalent to that between the vase and the solid body of flint, we must necessarily assume a very firm bond to the clay underneath, and I do not think we can consider that possible until vitrification has set in. I understand that the bulk of shrinkage takes place at the vitrification point, not before. So you have got to get your bond on the clay before vitrification; otherwise you will have a tear.

Professor Orton: I do not see it that way. It seems to me the shrinkage undoubtedly takes place *during* the process of vitrification, and there is no need of a bond previous to that point, except as the drying shrinkage may require it, and from the minute you begin firing until vitrification sets in, there is little change in volume. If the slip and stoneware were separate, they would probably shrink differently, but being fastened on each other, the bond develops by vitrification between the two fast enough to neutralize the strain produced by this disparity in shrinkage rate which would occur if they were free. That is the way I explain it — it is a sort of progressive attack on the body, as well as porgressive change in the slip itself.

Mr. Burt: I feel that Prof. Orton's explanation is undoubtedly correct. If we make a cone from a glaze mix and completely fuse it, we have as a result a small drop of glaze. Take the same amount of glaze, spread it on a piece of ware, fire it, and we have this glaze extended over a large space. This seems to me much the same phenomena that we have in the vitrified slip. We may apply our glaze to a burnt piece or to one of raw clay. It will hold equally well. The same is true of an engobe. It may be applied on the clay or on the biscuit, and will hold equally well in either case, provided it is properly compounded. It is absolutely necessary, however, that there be a proper mechanical bond between body and slip, or body and glaze, when vitrification sets in. If this does not exist, a glaze will crawl and an engobe peel.

Seger says, that in adjusting an engobe to a body, the rational analysis of each should be carefully considered. I had hoped Mr. Geijsbeek would give us his experience on this subject.

Professor Binns: I think there is an interesting connection between that experiment and the general practice in porcelain factories where flint is commonly used to hold the shape and prevent shrinkage of the ware. And it would be an interesting study, in this example, to make a microscopic examination of the structure of the two vases. It seems to me possible that the larger body is less dense than the

smaller. But it shows the flexibility in vitreous bodies which can be influenced by mechanical means. I think if the fire had been carried a little further the larger vase would have burst. I also think it probable that cone eight heat was not thoroughly reached in the center of the bed flint. You know that heat penetrates slowly into the center of such a mass. You could not know whether that temperature was in the center of the flint unless a pyrometer could be inserted.

Mr. Stull: I think the comparison I made was a fair one. Both vases were fired in a small sagger and in the same kiln and under similar conditions. Even if the cones did not correctly represent the temperatures, the latter were at least comparable.

Mr. Everett F. Townsend: I would say that there is about the difference of one cone between putting tile in a sagger with sand, and putting it in without sand. The shrinkage in the two cases differs by about the same amount as that occasioned by one cone's change in temperature.

STONEWARE GLAZES.

BY

ROSS C. PURDY, COLUMBUS, OHIO.

The term stoneware, as popularly used in this country, means a grade of pottery made from a natural clay or mixture of clays, which is burnt to vitrification, and in which the body and glaze are matured in one and the same fire. The articles usually made are crude jugs, crocks, receptacles for fruits and dairy products; latterly the line has been increased to include many kitchen, table and toilet articles; in addition, a small quantity of stoneware is employed by the chemical industries, for receptacles, stills, worms, stopcocks, conduits, strainers, and similar wares. This constitutes no item in the general trade, but brings prices far above those of general stoneware. In this country, however, the stoneware has not yet attained the reputation for excellence that is enjoyed by the Flemish, German or Japanese products.

The nature of the body used in the manufacture of foreign stoneware is unknown to the writer, but two receipts given as German stoneware bodies, were found to read as follows:

I.	English Blue Clay	25%	II.	Plastic Fireproof Clay	36%
	Kaolin	32		Kaolin	30
	Flint	35		Flint	30
	Spar	8		Spar	4

The American stoneware, with perhaps one or two exceptions, is made from either a single low-grade or No. 2 fire clay, or a mixture of two or more such clays. In nearly all cases the body may be said to be a native, yellow burning fire clay, containing enough fluxes to vitrify it to a blue-gray dense body at about cone 8. It is exceptional, if at all, that a prepared body like those cited is used.

I have also seen German stoneware glazes that figure out to be high-fire fritted glazes. An illustration of these glazes is as follows:

Fritt —	Red Lead	9.1%
	Quartz	32.7
	Borax	27.3
	Whiting	18.2
	Kaolin	12.7
		100.0

Glaze —	Fritt	80%
	Spar	12
	White Lead	8
		100

On calculation, the formulæ of fritt and glaze are found to be respectively:

$$\text{Fritt} - \left.\begin{array}{l} 0.136\ PbO \\ 0.244\ Na_2O \\ 0.620\ CaO \end{array}\right\} 0.168\ Al_2O_3 \left\{\begin{array}{l} 2.20\ SiO_2 \\ 0.49\ B_2O_3 \end{array}\right.$$

$$\text{Glaze} - \left.\begin{array}{l} 0.20\ PbO \\ 0.21\ Na_2O \\ 0.53\ CaO \\ 0.06\ K_2O \end{array}\right\} 0.20\ Al_2O_3 \left\{\begin{array}{l} 2.23\ SiO_2 \\ 0.42\ B_2O_3 \end{array}\right.$$

The nature of the body and glaze, as given, suggests a ware of much higher grade than any American stoneware. I know of only one instance where an American stoneware potter uses a lead glaze, and I do not know of any that use a fritted glaze.

It must be that in Europe, "Stoneware" includes higher grades of ware than is generally accepted as such in this country.

The nature of the body, as well as the single burn with its slow heating and cooling, precludes the use of lead and boracic glazes from a chemical standpoint. The low price obtained for the ware and the consequent economy of manufacturing, necessitates a glaze that will mature with the body, giving a smooth, glossy coating without having flowed, and which will not be contaminated by direct contact with the fire gases.

Such glazes as are used by American stoneware potters may be classified as follows:

I. Salt Glazes
II. Slip Glazes
III. Bristol Glazes { Clear, White, Colored

SALT GLAZES.

Salt glazing, while fast disappearing from use in the stoneware industry today, is worthy of consideration because of its very interesting chemical reactions and its past importance.

To be salt glazed, the ware is set more open than the ware which is coated with a slip or bristol glaze, so as to expose all the surfaces to be glazed to the free action of the fire gases. The inside of the pieces are glazed with a slip glaze, and are not exposed to the action of the salt vapors.

The heat attained is that sufficient to almost vitrify the body. This is essential in successful salt glazing, for the alkali fumes would penetrate a soft porous body and not give the desired fluxed or glazed surface on the ware.

When the trial pieces drawn show that the body has attained the required density, and the fires have burned clear, salt is thrown onto the fires, either a trifle damp or with wood. Fumes of Na Cl and water vapor are thus generated, the water vapor decomposing the fumes of sodium chloride, setting the alkali free in the kiln to attack the exposed surface of the ware and fusing it into a thin coat of glaze.

The glaze thus formed must naturally approach the formula of slip and Bristol glazes, deriving the necessary Al_2O_3 and SiO_2 from the body. For successful salt glazing the clay must have approximately the same proportion of Al_2O_3 to SiO_2 as is found to exist in the slip and Bristol glazes, viz: from 1:5.5 to 1:7.

SLIP GLAZES.

Slip glazing is the application to the surface of the ware of such clay or clays as will fuse and form a glaze at the heat sufficient to vitrify the body. Such fusible clays are of the ferruginous type, burning to either a red, brown or black.

The Albany slip is widely known for its uniformity in point of fusion and in color. It is a clay mined near Albany, N. Y. Its chemical formula when burned, as given by Langenbeck, is

$$\left.\begin{array}{l}0.1954\ K_2O\\0.4592\ CaO\\0.3454\ MgO\end{array}\right\}\left.\begin{array}{l}0.6080\ Al_2O_3\\0.0810\ Fe_2O_3\end{array}\right\}3.965\ SiO_2$$

Slip clays possess some subtle quality given by nature that render them impossible of duplication by mechanical mixtures of the same formulæ. Starting with a ferruginous clay as a basis, adding such ingredients as to duplicate the formula of the natural slips exactly, one would not obtain a glass like or equal to that of the slip glaze.

Albany slip produces a glaze that stands well upon almost every clay body, prepared or natural, without crazing or dunting. It has been observed that lead glazes containing 50 per cent. or more of Albany slip will stand a greater variation in the body than will a lead glaze without the slip clay. What that subtle factor is which renders this clay so valuable and which refuses duplication by mechanical means, is not known.

BRISTOL GLAZES.

A Bristol glaze is one composed essentially of silicate of the alkalies, alkaline-earths and alumina, applicable to green ware, and capable of being burned in open fire with slip glazes. Bristol glazes may be either clear and colorless, white, or colored. The study of clear and colored Bristol glazes will not be taken up at this time.

WHITE BRISTOL GLAZES.

The white Bristol glaze from a scientific standpoint, is a new field for research. Its application is so recent that the ceramic chemist has had hardly time to analyze the scientific causes for its various behaviors, or to ascertain the workable limits of its chemical formula for practical work.

I have not been able to myself carry out any extended research on this subject, but I have at hand the record of a large number of experiments made by a former assistant, with whose methods of work I am familiar, and in addition, I have the trial pieces numbered to correspond to the record. While I would have planned my series differently in some respects, if attacking the problem at the beginning, I still feel that a good deal of value can be gotten from the study of the matter already at hand.

The following notes are therefore to be considered as a digest of these experiments, supplemented by such opinions as my own experience has confirmed in the premises:

I. Oxygen Ratio in White Bristol Glaze.

In the hope of eliciting facts by comparing the oxygen ratio and the ratio of Al_2O_3 to SiO_2 in the glazes of which I have samples at hand, I calculated out the ratios and tabulated them. While it was not found that any definite oxygen ratio, and silica alumina ratio could be said to be the best, it was evident that the best Bristol glazes had an oxygen ratio, ranging between 1:2 and 1:2.5, and silica alumina ratios ranging between 1:5 and 1:7.

From Table I, given later, it is to be noted that even with the oxygen ratio with the limits that seem to be best, the glazes are not good because of physical peculiarities. This illustrates the fact that a Bristol glaze stands between the lead glazes, that are under definite chemical control, and ceramic bodies and pastes that obey chemical laws in but a small part, Chemically, we can increase or decrease the fusibility of a clay body, but the physical properties such as shrinkage, etc., are beyond chemical calculation. It can be said that this or that oxygen ratio is the best in lead glazes, but such is not the case in Bristol glazes.

However, the trials in table I, whose oxygen ration are within the limits of 2—2.5 are better matured and have a better gloss than those of higher or lower oxygen ratio.

II. Al_2O_3 in White Bristol Glazes.

Al_2O_3 in Bristol glaze performs the same function as in lead glazes, being an intermediary element between the fluxes and the acid. In addition to this chemical function, it has a physical function which enters but little into that of a lead glaze, viz: regulation of shrinkage and maintainance of a viscous paste.

It was believed that crawling and pinholing was more prevalent where the Al_2O_3 was high. Desiring to obtain the evidence indirectly, a number of trial pieces showing these defects were chosen and their corresponding formulæ, oxygen ratio, and silica-alumina ratio tabulated.

TABLE I.

No. of the Glaze	Equivalents of the Ingredients.					Oxygen Ratio	Silica-Alumina Ratio
	K_2O	ZnO	CaO	Al_2O_3	SiO_2		
21	0.444	0.444	0.111	0.555	4.44	3.333	8.00
22	0.50	0.388	0.111	0.6	4.44	3.13	7.26
23	0.555	0 333	0.111	0.666	4.44	2.90	6.66
113	0.50	0.40	0.10	0.55	4.00	3.01	7.27
114	0.55	0.40	0.05	0.6	4.50	3.21	7.50
177	0.50	0.40	0.10	0.6	3.21	3.28	5.33
178	0.50	0.40	0.10	0.65	3.30	2.23	5.07
179	0.50	0.40	0.10	0.70	3.40	2.20	4.85
180	0.50	0.40	0.10	0.75	3.50	2.14	4.66
199	0.50	0.40	0.10	0.55	4.00	3.01	7.27
231	0.50	0.40	0.10	0.55	3.25	2.45	5.90
347	0 50	0.40	0.10	0.70	3.60	1.16	5.14
424	0.40	0.45	0.15	0.55	2.93	2.22	5.36
443	0.50	0.40	0.10	0.60	3.61	2 57	6 00
444	0.50	0.40	0.10	0.60	3.80	2.70	6.33
445	0.50	0.40	0.10	0.60	4.00	2.85	6.66
446	0.50	0.40	0.10	0.65	3.60	2.44	5.33
447	0.50	0.40	0.10	0.65	3.80	2.57	5.84
448	0.50	0.40	0.10	0.65	4.00	2.71	6.15

As it is impossible in commercial work to glaze all the ware when at "leather hard" condition, it has become the general practice now to dip the ware when "bone dry," although the finished ware is better if dipped when "leather hard." These trials were dipped when "bone dry."

These trials bore evidence to correctness of the assumption that crawling, in most cases, is due to too much clay. Too much clay means too much shrinkage in clay state, either on drying floor, or in kiln, or both. Too much shrinkage causes cracks in the glaze, perhaps not perceptible at casual glance of the operator, but which develop during the early stages of the burning, leaving on the burned ware a bare spot or seam. As the body exposed by these bare spots and seams does not show evidence of having been affected by any fluxing action, it is safe to presume that the glaze had crawled before it had been fused even to the slightest extent.

.55 equivalent Al_2O_3 seems to be the maximum that is safe to use under the existing conditions.

To see if the converse was true, a few trials, free from crawling and pinholding were selected, and their corresponding formulæ found to be as follows:

TABLE II.

No. of Glaze	Equivalents of the Ingredients.					Oxygen Ratio	Silica-Alumina Ratio
	K_2O	ZnO	CaO	Al_2O_3	SiO_2		
54	0.233	0.4	0.366	0.33	1.94	1.3	6.00
106	0.45	0.3	0 25	0.50	3.50	2.8	7.00
107	0.50	0.3	0.20	0.55	4.00	3.01	7.27
110	0.50	0.35	0.15	0.55	4.00	3.01	7.27
111	0.40	0.40	0.20	0.45	3 00	2.55	6.66
116	0.40	0.40	0.20	0.50	4.00	3.20	8.00
570	0.45	0.40	0.15	0.50	3.00	2.40	6.00
575	0.45	0.40	0.15	0.525	3 15	2.44	6.00

There were other trials free from crawling and pinholing, but as they had the same Al_2O_3 and SiO_2 equivalents as those here represented, they were not figured out.

It was observed, also, that when the Al_2O_3 exceeds .55 equivalents, the glaze loses the *clear white* color and takes on a *grayish white* tint.

A Bristol glaze without any equivalents of clay, strange to say, has not the fusibility of those in which .05 equivalents of clay is used, as will be seen by the following:

TABLE III.

No.	K_2O	ZnO	CaO	Al_2O_3	SiO_2	
167	0.5	0.4	0.1	0.5	3.00	All Al_2O_3 derived from Spar
168	0.5	0.4	0.1	0.5	3.25	
169	0.5	0.4	0.1	0.5	3.50	
170	0.5	0.4	0.1	0.5	3.75	
171	0.5	0.4	0.1	0.5	4.00	

TABLE IV.

No.	K_2O	ZnO	CaO	Al_2O_3	SiO_2	
172	0.5	0.4	0.1	0.55	3.00	.05 Al_2O_3
173	0.5	0.4	0.1	0 55	3.25	derived
174	0.5	0.4	0.1	0.55	3.50	from Clay,
175	0.5	0.4	0.1	0.55	3.75	balance
176	0.5	0.4	0.1	0.55	4.00	from Spar

This being a surprise, the samples are submitted for inspection. Those containing the clay are very distinctly better than those without it.

In the Bristol glaze used the most extensively in the Muskingum Valley district of Ohio, the equivalent of clay averages about .14, the glaze maturing at cone 8 to 9.

III. K_2O in White Bristol Glazes.

Potash is generally the only alkali flux used in Bristol glazes. Soda is sometimes introduced by the use of soda spar or ground glass, but these materials are seldom used. The soda introduced by the small amount of borax used in some cases, is too small to exert any important influence.

In feldspar, the stoneware potter finds the potash in the best and cheapest form for his use.

The practical limitations of the use of feldspar is governed on the one hand by the amount of Al_2O_3 permissible in the formula, and by the solvent action of the potash upon ZnO ; and on the other hand by the potash required to flux the glaze to a bright gloss at the heat used.

Lead, as a flux in a white Bristol glaze, is not permissible, not only because of its poisonous character, but because of its great solvent action upon ZnO.

The empirical experimenter in increasing or decreasing the fusibility of his Bristol glaze generally attempts to merely raise or lower the percentage amount of feldspar. In some cases, such a course of precedure would bring the desired results. But through chemistry we learn the limits of such variations.

From the formulæ having .55 equivalents of Al_2O_3, which was found to be the maximum that could be worked with safety, the following were selected as being the best:

TABLE V.

No.	Equivalents of the Ingredients.					Oxygen Ratio	Silica Alumina Ratio
	K_2O	ZnO	CaO	Al_2O_3	SiO_2		
172	0.5	0.4	0.1	0.55	3.10	2.34	5.63
173	0.5	0.4	0.1	0.55	3.25	2.45	5.90
174	0.5	0.4	0.1	0.55	3.50	2.64	6.36
175	0.5	0.4	0.1	0.55	4.00	3.07	7.27
176	0.5	0.4	0.1	0.55	4.00	3.07	7.27
199	0.5	0.4	0.1	0.55	4.00	3.07	7.27
231	0.5	0.4	0.1	0.55	3.25	2.45	5.90

As each of the trials in this table was weighed, ground and applied independent of any other, all possibility of an error influencing each one alike is eliminated. Each stands for itself.

It is noted that .5 K_2O is found necessary to bring these glazes to a good maturity at cone 5 to 6, the heat at which all of the trials were burned.

To harden the glaze, lower the K_2O and increase the CaO, but do not alter the Al_2O_3.

IV. ZnO in White Bristol Glazes.

Zinc has a double mission in a Bristol glaze, acting both as a flux and an opacifier. Other than SnO_2, it is the best opacifying element known to the potter. It is a good flux in lead glazes, adding brilliancy to colors. It is also a good flux in Bristol glazes, making a more fusible RO combination with K_2O and CaO, than would K_2O and CaO alone.

It is astonishing to many that ZnO in some Bristol glaze formulæ does not act as an opacifier, but is completely dissolved, leaving the glazes perfectly clear.

Two glazes showing this are here quoted:

TABLE VI.

No.	K_2O	ZnO	CaO	Al_2O_3	SiO_2	
425	.45	.40	.15	.60	3.25	White
466	.55	.35	.10	.45	2.80	Clear

All the glazes that were either clear or but slightly opaque, were selected and tabulated as follows:

TABLE VII.

No.	K_2O	ZnO	CaO	Al_2O_3	SiO_2	Oxygen Ratio	Silica Alumina Ratio
105	0.40	0.3	0.3	0.45	3.00	2.55	6.66
106	0.45	0.3	0.25	0.50	3.50	2.80	7.00
107	0.50	0.3	0.20	0.55	4.00	3.01	7.27
108	0.40	0.35	0.25	0.45	3.00	2.55	6.66
109	0.45	0.35	0.20	0.50	3.50	2.80	7.00
110	0.50	0.35	0.15	0.55	4.00	3.01	7.27
115	0.40	0.35	0.25	0.50	4.00	3.20	8.00
458	0.30	0.35	0.35	0.40	2.50	2.27	6.25
459	0.325	0.35	0.325	0.425	2.65	2.30	6.33
461	0.375	0.35	0.275	0.475	2.95	2.43	6.20
460	0.350	0.35	0.300	0.45	2.80	2.38	6.22
462	0.400	0.35	0.25	0.500	3.10	2.48	6.20

It is noted that, irrespective of the equivalent amount of the other elements, the resultant glaze is either clear, or nearly so, when the ZnO is .35 equivalent or less.

To note the effect of the use of more than .4 ZnO, the following were compared:

TABLE VIII.

No.	K_2O	ZnO	CaO	Al_2O_3	SiO_2	Oxygen Ratio	Silica Alumina Ratio
117	0.40	0.45	0.15	0.50	4.00	3.20	8.00
118	0.40	0.50	0.10	0.50	4.00	3.20	8.00
424	0.40	0.45	0.15	0.55	2.95	2.22	5.36

Of these, 117 is very good except that it is not perfectly matured. It showed no evidence of crawling. The other two crawled.

It is safe to assume, then, that when the equivalent of Al_2O_3 is 0.5, 0.45 equivalent of ZnO can be safely used, but with 0.55 equivalent of Al_2O_3 not more than 0.4 equivalent of ZnO can be used.

To zinc, many ascribe all the responsibility for the manifestations of crawling, but we have not found that to be the case. ZnO and Al_2O_3 alike are responsible and their

workable limits have here been found to be between 0.50 to 0.55 equivalent Al_2O_3 and 0.45 to 0.40 equivalent of ZnO. When the Al_2O_3 is high, the ZnO should be low, and vice versa.

V. CaO in White Bristol Glazes.

CaO is added as the third RO element. It acts in most cases as a flux, but in some it acts as an opacifier also, or at least adds to the opacity of the glaze.

CaO is an alkaline earth, practically infusible by itself, but in a silicate mixture it is readily soluble with a lead flux, and at higher heats, with the alkali fluxes.

It is used in the compounding of a Bristol glaze in the form of ordinary whiting. In some cases, it is derived also from CaF_2, and $Ca_2(PO_4)_2$, but these cases are rare.

The writer has made no experiments with $Ca_2(PO_4)_2$ as a source of CaO but knows of instances where it is used. It is said to add opacity to the glaze.

Fluorspar is an ingredient that needs to be added very cautiously. Its specific weight makes it a troublesome factor in keeping the glaze mixture homogenous in the dipping tubs.

The following trials will show to some extent its chemical action :

TABLE IX.

No.	K_2O	ZnO	CaO	Al_2O_3	SiO_2
562	0.55	0.35	0.10 CaF_2 0 00 $CaCO_3$	0.65	3.75
563	0.55	0.30	0.15 CaF_2 0.00 $CaCO_3$	0.65	3.75
564	0.55	0.35	0.05 CaF_2 0.05 $CaCO_3$	0.66	3.75
565	0.55	0.30	0.05 CaF_2 0.10 $CaCO_3$	0.65	3.75
566	0.55	0.30	0.10 CaF_2 0.05 $CaCO_3$	0.65	3.75
567	0.45	0.40	0.05 CaF_2 0.10 $CaCO_3$	0.60	3.25
568	0.45	0.40	0.10 CaF_2 0.05 $CaCO_3$	0.60	3.25

As to color, these trials are whiter and glossier than were those of similar composition, but in which all the CaO was obtained from $CaCO_3$.

Samples of these glazes were so placed as to receive a soft and a hard burn. In the softer burned samples, the pinholes, while distinct, are not so numerous as those on the harder burned samples. This same action of CaF_2 was noticed in vitreous tile bodies.

These trials would indicate that 0.05 equivalent of CaF_2 is the maximum that can be used in these glazes. and that for higher heats (cone 7 and up) its use in vitreous or fusible ceramic mixtures is not permissible.

It is to be noted also, that even with the high equivalent of Al_2O_3 these trials show no tendency to crawling.

The glazes given above contained 0.01 equivalent of of borax. The following glaze was made up without borax as follows:

.25	Eqv.	Feldspar.
.20	"	Common Glass.
.05	"	Whiting.
.25	"	Fluorspar.
.15	"	Magnesia.
.10	"	Zinc Oxide.
.10	"	Clay.
.30	"	Flint.

The formula thus produced is:

$$\left.\begin{array}{l} 0.25\ K_2O \\ 0.10\ Na_2O \\ 0.40\ CaO \\ 0.15\ MgO \\ 0.10\ ZnO \end{array}\right\}\ 0.35\ Al_2O_3\ \ 2.5\ SiO_2$$

This glaze was burned in the open kiln at cone 8. It did not pinhole in the least, and even with the low ZnO and Al_2O_3, was a fair white Bristol glaze.

VI. SiO_2 in White Bristol Glaze.

SiO_2, as an acid, has the same functions in a Bristol glaze as in a lead glaze. However, as Bristol glazes are not designed to flow, as are the lead and boracic glazes, but rather to be only thoroughly fused to a viscous paste, the acid does not have quite the same influence. Indeed, it will be seen that the best Bristol glazes have a lower proportion of Al_2O_3 to SiO_2 than do the ordinary lead glazes.

While SiO_2 is almost infusible by itself, it is often the case that a glaze is too refractory because of its low acidity, and to soften it, more SiO_2 must be added. The converse is likewise true.

As will be seen by the following formulæ of Bristol glazes known to be in use in factories in different parts of the country, consequently on different clays, the best ratio for alumina to silica is between 1:5 and 1:7:

TABLE X.

FORMULÆ	Salts added to prevent crawling	Cone to which Glaze is Fired	Silica Alumina Ratio
$\left.\begin{array}{l}0.407\ K_2O\\0.194\ CaO\\0.399\ ZnO\end{array}\right\}0.507\ Al_2O_3\left\{2.857\ SiO_2\right.$		5–6	5.63
$\left.\begin{array}{l}0.377\ K_2O\\0.180\ CaO\\0.444\ ZnO\end{array}\right\}0.516\ Al_2O_3\left\{2.84\ SiO_2\right.$	0.015 Borax	8	5.5
$\left.\begin{array}{l}0.450\ K_2O\\0.150\ CaO\\0.400\ ZnO\end{array}\right\}0.6\ Al_2O_3\left\{3.25\ SiO_2\right.$	0.01 Borax	5–6	5.4
$\left.\begin{array}{l}0.327\ K_2O\\0.300\ CaO\\0.370\ ZnO\end{array}\right\}0.366\ Al_2O_3\left\{2.54\ SiO_2\right.$	3% Borax	6	6.9
$\left.\begin{array}{l}0.300\ K_2O\\0.200\ CaO\\0.500\ ZnO\end{array}\right\}0.5\ Al_2O_3\left\{4.00\ SiO_2\right.$	.05 Boracic Acid	10-12	8
$\left.\begin{array}{l}0.224\ K_2O\\0.388\ CaO\\0.386\ ZnO\end{array}\right\}0.3567\ Al_2O_3\left\{3.00\ SiO_2\right.$		7	8.4

Having found that for the conditions under which the trials quoted were made, the K_2O, ZnO and CaO work best when in the proportions respectively of 5:4:1 equivalents, with Al_2O_3 at 0.55 and a silica-alumina ratio of about 1:6, then the following formula may be regarded as the type or standard to be used under like conditions:

$$\left.\begin{array}{l}0.5\ K_2O\\0.4\ ZnO\\0.1\ CaO\end{array}\right\}0.55\ Al_2O_3\left\{3.30\ SiO_2\right.$$

which will develop into a good white glaze at cone 6.

VII. Common Defects in White Bristol Glaze.

I. Crawling.

Crawling may due to any one of the following causes or to the combined inflnence of any two or more of the conditions, even though they may be present in but a slight degree.

A. Too high in clay, or Al_2O_3

B. Too high in ZnO

C. Too thickly dipped

D. Dusty ware

E. Sweating in drying, or early stages of the burning.

"A" and " B " have been discussed.

" C " is obvious to those who have ever coated green ware with another clay mixture.

" D ", while it does not receive much attention from the average stoneware potter, is, nevertheless, the cause of much imperfect ware. Electrical insulating companies who make dry press ware have this trouble to a larger entent than do the stoneware companies.

"E" is better seen by the following citation:

For economy of kiln space, and convenience as well, coffee pot lids were often placed between kettles after having been dipped, and hacked in bungs of from 20 to 25 each. The kettles and lids contained such moisture as was absorbed in dipping. These bungs were left on the open drying floor for three or more days. The bungs were then carried intact to the kiln.

It was observed that on over 80 per cent. of those lids, the glaze would be peeled in places and in some cases slipped off the lid entirely. That same glaze under normal conditions of drying, worked beautifully, having very little crawling.

The crawling in this case was obviously caused by the thorough sweating these lids must have been subjected to, being between the crocks where no ventilation was possible.

It is a fact that in a steam dryer with fair ventilation, tile containing from 6 to 10 per cent. of moisture when

placed within it, will sweat until they appear to be very wet, after having been there 24 hours.

The case just cited, while an extreme case, illustrates the effect which too little draft in the "drying off" stage of burning will have.

Most stoneware potters insist upon "keeping the kiln hot", that is not to allow them to stand longer between the burnings than necessary to draw and set. It is the experience of potters, known to the writer, that if the kiln is cool when set, crawled glaze can be depended upon.

This is obviously another case of sweating. The kilns have drawn moisture, which with the moisture from the ware, causes sufficient steam to thoroughly sweat the ware.

As an antidote for crawling, some soluble compound such as $Na_2B_4O_7$ or Na_2CO_3 and Plaster Paris slop are used. To what the counteracting effect of these compounds may be due, is not positively known. Some contend that in drying the soluble salts crystalize, so interlocking themselves as to bind the whole into a coating that will not be apt to crawl.

II. Pinholes.

Pinholes are more often due to imperfect finishing of the ware. Small cavities left in the surface of the ware, become filled with air when the piece is dipped. The air is expelled, blowing a hole through the glaze that has covered the cavity; the result being a pinhole in the finished product.

III. Dryness.

Dryness may be caused by two conditions.

First. By too rapid raising of the heat in the early part of the burning.

Second. By too low or too high SiO_2 as in the last formula given as being known to be in use.

IV. Dusting

After the ware has dried, it is rubbed by the hands and clothes of the kiln setter and if the glaze is too dry, enough will be rubbed off to cause defective ware.

Dusting is prevented by the use of Ball clay in place of China clay or $Na_2B_4O_7$ in place of plaster slop, or use the plaster paris before it has had time to harden.

DISCUSSION.

Mr. Otto Hensel: Last year there were some experiments presented here regarding the use of magnesia as a flux, but no special attention was called to the value of magnesia as to other properties desired in a glaze, such as prevention of crazing and preservation of color. If anything was said on this point, it was mentioned incidentally, but it was not stated that magnesia is more valuable than lime in making a glaze having a greater range of use. In Germany they are beginning to pay more attention to the value of magnesia than they did before. As regards magnesia in clays, from another aspect, Dr. Mäckler states in a technical periodical, that he has subjected some fireproof terra-cotta lumber to chemical analysis. These particular hollow blocks are imported into northern Germany from southern Europe, and are in great favor because they have very thin walls and are nevertheless quite true in shape. He inclines to the belief that their high content of magnesia, estimated at about 9 per cent. carbonate of magnesia in the raw state, gives properties to the clay, such as lime alone will not furnish. He announces that he is going to make a further report on the effects of magnesia. I have noticed that clays in which magnesia is as high the lime are very valuable in holding up their shape in firing, and are perhaps of better quality when burned than if they had been lime in place of the magnesia present.

Professor Orton: Mr. President, I want to say, in corroboration of what Mr. Hensel has called attention to, viz: that the presence of magnesia may be a source of peculiar value to the potter, that I made a series of tests in the way of regular class work, in which we attempted to make a slip like the Albany, beginning with common red shale as a base, and adding those ingredients which the shale did not have in itself. It was not intended as a practical process, because it is cheaper to buy Albany slip than to make it; but it occurred to me as a good problem in the way of drill in the preparation of ceramic mixtures. So we proceeded to alter the red clay to make it do what Albany slip would do as nearly

as we could. In that series of tests, which were only moderately successful, we used whiting to supply the lime and, incidentally magnesia. We construed the lime and magnesia as equal and used lime in place of magnesia; and we were constantly troubled to get anything like the desired color effects where the iron used was merely equal to that in the Albany Slip. My class in experimental work is repeating the work which we did several years ago, with a different clay. We are taking red Bedford shale which is better suited because of more alkalies and more iron; and we are doctoring this red shale into as close an approximation of Albany slip as we can, but we are using dolomite as a source of lime. While we have no results as yet to bring forth, I believe we are rather in the way of progress there, because I believe that magnesia does have a different effect on the color of iron; or, rather, it fails to mask the effect of iron, as lime alone does.

I want to say that this paper has been a surprise to me. Mr. Purdy has been changing his location from Ohio to Illinois, and the paper has been prepared in the last three weeks, in intervals of moving. He has put together more concrete information on the subject of stoneware glazes than I have seen in any one place before. And, further, the results he exploits, while not prepared, perhaps, in the way of a regular series, beginning with the changing values of one element at a time, are a large number of them glazes in actual use in representative stoneware factories at the present time, and gathered by him from a wide knowledge among stoneware men themselves; and I submit that when the paper is put out in printed form, it will be a compendium for stoneware men which they should be exceedingly glad to get, if they are wise. Furthermore, I believe that the same class of glazes are capable of a wide extension in use by enameled brick men. I believe it is true that there is no cheaper glaze with which to coat an architectural product than those of this general lime; therefore, to enamel brick men and terra-cotta men, I think this paper is of very substantial value.

Mr. Burt: My attention was called some time ago to the fact that a high content of zinc in a glaze appeared to render the glaze less acid proof. Four-tenths equivalents of ZnO is rather a high content. Have any of the members experience enough to say whether that amount does carry with it some danger as to the stability of the glaze, as to being acid proof?

Mr. Arthur S. Watts: I have had a little experience in that line. I used for four-tenths of zinc on a glaze for porcelain, fired at cone twelve, single burn for body and glaze. It was repeatedly tested with acid, and never showed any effect of attack. Of course, the temperature used was high. Whether at a lower temperature like ordinary stoneware heat, it would be affected, I do not know.

NOTE ON FRITTING.

BY

STANLEY G. BURT, CINCINNATI, OHIO.

One of the first things I was told on entering the pottery business, was that fritting reduced the contraction of a glaze, and therefore tended to stop craze. I have seen this statement printed many times since. It sounds very plausible. Crazing is undoubtedly due to an excessive contraction of the glaze. Then, if fritting reduces contraction, as it undoubtedly does, why doesn't it stop crazing? I quote from a recently published article on pottery:

"The preparation of the glaze is quite as important, for it is composed of materials subject to great contraction, namely, quartz, feldspar, paris white, borax and a little lead. These, with the exception of the latter article, are fritted or melted in an oven till they run like molasses, they then harden and are crushed and ground in water. Having lost much of their contractility, they are now ready to coat the ware and will fuse and harden on the biscuit without crazing."

As I said in my paper last year, we have in the pottery business two distinct contractions which I called permanent contraction or shrinkage, and constant contraction. If we fire a piece of clay until it gradually approaches fusion, the pores close and we say the clay shrinks. This is permanent contraction. When once fired it can never go back to its original size. Now, if the fired piece is heated it will expand, and on cooling will contract to its usual size. This I call constant expansion and contraction; it is going on all the time, and for the same degree of heat is a constant quantity.

Much the same thing occurs in fritting a glaze; it loses its water of crystallization, carbonic gas, etc.; it fuses, the

pores close, and we have permanent contraction: that is, shrinkage; It has undoubtedly undergone a great permanent contraction, but what has this to do with the coefficient of its constant contraction? If we melt a glaze mix on the inside of a crucible, and call it a fritt, how is it different from the same mix fired at the same time on the outside of the crucible, and called a glaze?

So if we take a glaze mix and fritt it, this mix fuses and contracts; but this is merely permanent contraction, which, as explained, may have no effect whatever on the final coefficient of the glaze, so that a fritted glaze may craze as well as a raw glaze.

In statements like this, that have been handed down from generation to generation, there is generally some basis of fact, and I believe it is undoubtedly true that fritted glaze is less liable to craze than a so-called raw glaze. But, the reasons for this fact are entirely different from those assigned above.

Leaving aside the fact that boracic acid, one of the best means for eliminating craze, that is reducing the coefficient of the glaze, can only be used in a fritted glaze, and that almost all fritted glazes contain it. Let the same glaze be tested raw and fritted, and I think the fritted sample would undoubtedly show the lower coefficient.

A glaze is a chemical combination. All such combinations take more or less time for their completion. The perfect formation of a glaze takes quite an appreciable length of time. We do not, in firing, rush our kiln up to a maximum heat and then stop. We either approach gradually, or else hold the kiln at its full heat in order to allow time for the glaze to complete its chemical combination, or, as we say, its fusion. An under-fired glaze shows undissolved particles in suspension, but how is it with an over-fired glaze? In this case, having completely combined all the ingredients of the glaze mix, it begins to dissolve those of the body. Of the body ingredient readily taken into solution, flint is the chief, and this flint, taken from the body, reduces the glaze coefficient.

With a fritted glaze the chemical combination has already been completed, so that it fuses much more readily and starts at once, as it were, to take up flint from the body. So in the same interval of time, it will be able to take up much more than the raw glaze, and its coefficient will be brought lower and the danger of craze much reduced.

I feel it is peculiarily the duty of our society to explain pottery dogmas like this, and to make a clear understanding of the facts open to those who care to know them.

OPPORTUNITIES FOR RESEARCH IN A FACTORY.

BY

CHARLES F. BINNS, ALFRED, N. Y.

The common acceptation of the term "research" is, an original investigation which tends to increase the knowledge of mankind, but, while not neglecting this wide definition, the term may be employed as expressing a pursuit of information which shall increase one's own knowledge, even though others may have trodden the same path.

In this sense every experimenter is engaged in research and is adding to his personal store of facts, even though he discovers nothing new. At the same time, even the most modest research must be more than a mere trying of recipes. There are in the ranks of our clay workers, hundreds of men whose love for a recipe passes the love of women, but who never get further than the recipes can take them. The experimenting that is needed is along the line of testing the influence of various minerals and chemicals upon each other under differing conditions, and thus preparing the way for new combinations and perhaps important results.

But the question will be raised, "suppose a superintendent or foreman be successful in some discovery, of what use will it be to him, and will he be ready to impart his knowledge?"

I submit that the question is premature. The spirit of research slumbers in the minds of many. Perhaps it is starved by the exigent demands of other faculties or it may be drugged into quietude by pride or suspicion, for even clay-workers are human and have human failings; but let it be awakened, let an occasional hour be stolen from play and the draught of inquiry be tasted. Then shall the spirit of research awake and arise and difficulties will disappear as dew before the morning sun.

It has often been remarked that the nature of the work to be performed, measures the amount of time available for the performance. No young man, for example, ever pleaded want of time as an excuse for not calling on his lady love, nor will pressure of other work ever keep the devoted experimenter away from his pestle and mortar.

It is quite possible that no material gain — no money gain — may follow new discoveries or new applications of old rules, but he who gains knowledge gains power, and every success, even if unrecognized, places a man just so much further towards his goal. As to the imparting of knowledge, a clay-worker cannot make many trials, without feeling the need of some congenial spirit with whom to discuss the results, and a mutual interchange soon follows.

The common idea of the practical man — why should that expression not be changed? We are all practical men; the term seems, by common consent to belong to the man who has nothing but his practical qualifications to stand upon, no science, in other words — is that investigation is the prerogative of the scientific man, and the college professor. They are not practical, and have nothing to do but weigh and mix. To state that professors have no time for research, would be to controvert my own argument; but this I will say — after wrestling with classes of elementary students and patiently covering the familiar ground of formula and reaction, it is an immense relief to turn to the laboratory table and pursue some inquiry, thought out perhaps, in theory, during wakeful nights or such spare moments as may have been secured.

The experimenter in the factory has one great and far reaching advantage, viz: His kilns are being regularly fired. His trials for the most part are made at the established temperatures of his glost and biscuit ovens, and all he has to do when the trial is prepared is to hand it on to the kilnman.

Those on the other hand, who are popularly supposed to be able to experiment more easily, have to provide themselves with even the simplest appliances—to make their own saggers, to do their own potting, dipping and placing and to fire their own kilns.

The necessary tools for factory research are few and simple. A dozen jars, a pestle and mortar, a sheet of plate glass, a muller, a palette knife, a sponge and a couple of bowls with a good pair of scales make up the outfit. It is best to discard at once the use of pounds and ounces. A set of weighs from 100 grams to .1 gram will permit trials to be made in percentage proportions and the transitions to larger weights is easy. It is perhaps hardly necessary to point out the importance of system in trial making, yet a potter's table, covered with trials of every shape and size, with no distinguishing mark but an arbitrary sign, is familiar.

Clay trials should be made in a uniform mould or die, according to the ceramic field explored; the length should be upward of 10 cm. to allow that measure to be accurately marked as a test of shrinkage. The heat of the oven should be marked, either by anticipation in the clay or afterwards in color. A series number and a specific number should be given, and accurate notes kept. It often happens that a number of trials are thrown away after a few months, simply because the man who made them has forgotten what they were.

When fired, the briquette or tile can be glazed or enameled over one-half of its surface to allow a comparison between glazed and unglazed surfaces.

A corner of the mixing room can generally be set apart for this experimental work and to this spot, as to a haven of refuge after a stormy sea, the harassed superintendent can retire for a moment's breathing space.

It may not be out of place here to suggest some of the problems which may with profit be investigated by the practical potter. In the white ware industry the employment of native materials is an important question. A great deal of English kaolin is being used, and the only reason is that it has always has been so—a reason that might perhaps satisfy an Englishman, but which seems to be out of harmony with American ideas. The rational analysis of the respective clays can always be obtained, and the substitution becomes a matter of accurate adjustment. I am acquainted with potters who pride themselves on using nothing but

American clays, and yet there are others, making similar wares, who claim that these clays are not good enough.

Flint is a material about which little is known. Mr. Burt's paper of last year opened up the subject, and there is room for an exhaustive inquiry as to size of grain, shape of grain, dry or wet grinding and color. In glazes there is scope for unlimited research. Theoretically, many are satisfied as to the cause and cure of crazing, but how many have conducted a systematic series of experiments to support or disprove the theories? The best glaze for under glaze colors is a subject about which everybody wants information, and the best way to get it is to dig it out for one's self. Mr. Mayer gave an important contribution on this point at our first meeting, but the investigation should be pursued at other temperatures than that used by him.

In coarser wares, the problems are at least as numerous and important. Let me instance a few of them: The best structure and composition of a fire-brick to withstand fire and weight; the influence of grog, its proper composition and size of grain; the profitable production of enameled brick; the manufacture of blue paving brick; the successful manufacturer of electric conduits; the resistance of clay ware to electric pressure, and so forth.

No man can afford to stand still; others will not wait for him; but, by thoughtful work, it is possible for everyone to help towards the success of the industry which is his life's vocation.

The facilities of the factory are not to be despised. Even though there be less time for thought and research, less time is necessary for the worker who has a factory at his back can outpace him who has no such help.

If we could persuade each of our factory superintendents to be, even in a small way, an experimenter, and to tell of his work, a vast amount of useful ceramic information would soon be compiled to the great advantage of our industry and the benefit of every clay worker in the land.

THE COMPOSITION OF PORCELAIN FOR ELECTRICAL PURPOSES.

BY

ARTHUR S. WATTS, FINDLAY, OHIO.

Connection with and interest in the porcelain insulator business has impressed me with the fact that while the products of some companies stand very high tests and seem in every way ideal insulators, the products of some other companies are well nigh worthless.

What is the cause? What are the essential constituents of an electrical insulating porcelain?

Those who know, if any know, do not seem disposed to distribute the information. Hence, I have decided to pursue such a line of investigation as will determine, if possible, what these essentials are.

I give you my work along this line. The results as I find them may be disproven by later investigation, but if only a foundation for the later work may be laid at this time, I will consider myself well repaid for the time invested.

General Considerations on Electrical Porcelain Bodies.

I quote as follows, from Herzog and Feldman on the subject of electrical porcelain: "A porcelain mass will be the more resistant, the higher the proportion of kaolin and feldspar; also the finer the grain of the kaolin used, the higher will be the maturing point of the porcelain produced."

The leading materials used in porcelain manufacture are certain kinds of kaolin, which are very slightly plastic, quartz and feldspar. Of course ordinary feldspar displays basic properties in the fire, uniting with silica to form silicates, thus producing an intimate mixture of all silicates. Therefore, the contents of kaolin must be in a definite ratio to the quartz and feldspar in regard to the insulating properties.

Too much quartz makes the body too little plastic, thus making it difficult to work, and after burning, the ware shows impressions and some warping due to the tendency of the clay substance to the contract.

Too high a content of feldspar makes the body soft and liquid, and increases the tendency to crack.

A body low in feldspar does not produce sufficient vitrification and hence decreases its insulating efficiency.

A porcelain body may shrink 15 to 18 per cent., and it is very important that the exact degree of shrinkage be known. No crazing or shivering is allowable in the glaze used on ware for electrical materials. If the glaze is too infusible the ware will be dull, and if too fusible it is likely to be covered with blisters. Investigation has proven the condemnation of glazes containing metallic oxides to be unfair. Metallic oxides, when used in glazes combine with the silica, forming silicates.

Probably in no line of electrical investigation have greater progress and more varied experiments been made than in the shape and size of electrical insulators. The plain bell which is used for low potentials and which is also practical and satisfactory for alternating currents up to 1000 volts, does not meet the requirements of modern high voltage transmissions.

The first step in the way of improvement was made in 1857 by Borggreve. The product was satisfactory so far as electrical and mechanical requirements were concerned, but failed owing to the fact that the interior space was square, which gave rise to uneven tensions in the body and frequent fractures.

The second step in the way of improvement was the production of a very long insulator having a very narrow cross-section. It was the idea of the inventor to increase the resistance by means of a long arcing distance. The insulator produced was of clear glass and suffered from the contraction and expansion of the iron bolt used to connect it to the cross-arm and which became intensely hot from the sun's rays on the insulator. This condition caused the introduction in 1895 of the double bell insulator of porcelain.

In this, the interior cylindrical part of the insulator is covered by a second bell which prevents the radiation of heat and also the formation of dew.

Up to a tension of 3000 volts, the ordinary double bell 75 mm. wide and 100 mm. high suffices, but above this voltage the question of further improvement arises.

In 1869 Lenoir and Prudhomme obtained a patent for the use of oil in insulator bells. Insulators of this type were used in constructing the famous line between Lauffen and Frankfurt. They proved impractical, as the oil must be replaced frequently and in case of rain it was often replaced by water.

In constructing the Kaiser Wilhelm Canal Line a triple bell insulator without oil was used and proved satisfactory even when exposed to salt spray. This type is now commonly in use for high voltage transmissions.

At first the insulators were glazed and burned resting on the bottom edge of the bell, but the roughness of this edge seemed to cause the collection of drops of water and to prevent this, the insulator is now glazed all over, the glaze removed from the head and the insulator burned inverted. Very recently it has been discovered that by glazing the inside of the insulator in the threaded hole, the insulator is made much more resistant and need not be so thick.

The following table of the Delta Bell Insulator I take from Herzog and Feldman "Electric Lighting Installation:"

Weight of Insulator	Potential Voltage to be Carried	Height	Diameter	Shortest Distance Allowable		
				From Insulator to Nearest Object	Between two Parallel Lines	From Insulator to the Ground
grams		mm	mm	mm	mm	mm
1200	6000	115	145	200	400	190
2500	12000	150	190	260	520	220
4800	25000	170	250	360	720	280
7000	50000	225	250	460	920	370

The weight of the porcelain used in the insulator is not of the greatest importance, but rather, the quality of the porcelain and the kind of glaze.

Manufacturing Processes.

Just here a word regarding the working of porcelain bodies may not be out of place. In Europe, insulators are made by all five of the clay working processes, viz: turning, jollying, pressing, casting and dry pressing. I am familiar with all of these processes except casting, but am not prepared to pass judgment on the superiority of any given process of working, as of course the product and its shape and size would greatly influence the method chosen. Dr. Seger, in his work, shows very conclusively that a semi-dry process, in which the material to be pressed contains the highest per cent. of water allowing of good pressing, gives the densest and strongest body.

Turning of large insulators in done similarly to all clay turning, the product being made mouth up and the top finished when the ware is sufficiently dry to permit of its being inverted. In this class of ware, much trouble is experienced in making the screw-thread for the pin which is to support the insulator. This thread is made by means of a threaded steel tube which is provided with a handle on one end and which is slowly turned in and out. In place of a full steel tube a segment may be used. Automatic boring apparatus is now being applied for this work.

Jollying is now largely replacing turning although it does not insure as dense and uniform a body as the former process.

Pressing is done in molds. The clay is placed in the mold in a lump and by means of a plunger is forced to assume the shape of the inside of the mold for its exterior while the plunger shapes its interior.

Dry pressing produces more exact shapes than any other method. The molds used are of steel and are very expensive. The body must be in such shape that it will not stick to the mold. To obtain it in this condition some factories

use the following process: The clay after drying and being thoroughly pulverized, is placed in wet cement or plaster boxes and allowed to absorb 10 to 20 per cent of moisture. The mass is then sieved thoroughly. Ten kilograms of the body will contain about 1 to 2 litres of water. Of course this proportion must be maintained exactly in order to insure a uniform shrinkage. The molds are sometimes complicated and are often composed of insertions. The piece of ware produced is threaded by means of a screw inserted in the bottom before the clay is introduced and which is screwed out rapidly by means of a crank. The tendency is to make insulators without threads, as this has always been a great source of trouble and loss in manufacture.

Selection of a Standard Porcelain as a Basis for the Experimental Work.

In outlining the work that follows, it seemed necessary to choose some type or standard of porcelain which I could use as a basis from which to map out varying series. The wisest course here seemed to be to procure a large number of standard porcelains, and to obtain from these by comparison the most characteristic type of porcelain now in use.

In compiling the following list of porcelains, I have not stated the present use of the body, but will state that the list includes only practical workable porcelains that have been, or are now, in actual use.

I wish to further state, that in my opinion such a list is valuable as a comparison and not as a basis from which to compound practical porcelains. Acting on Professor Binn's suggestion,* I shall take Al_2O_3 as unity in these formulæ.

* The Birth of English Porcelain, Trans. A. C. S., Vol. III, page 144.

TABLE I.

No.	Chemical Formulæ				Locality
1	.0467 K_2O	1. Al_2O_3	5.976 SiO_2		Japanese Body
2	.1140 K_2O	1. Al_2O_3	4.168 SiO_2		Society of Hal. Belgium
3	.1160 K_2O	1. Al_2O_3	3.75 SiO_2		Plymouth Body
4	.1213 K_2O	1. Al_2O_3	4.32 SiO_2		Lahalleur Vierzon (Heavy)
5	.0707 K_2O .0860 CaO .1567 RO	1. Al_2O_3	3.22 SiO_2		Bristol Body
6	.1622 K_2O	1. Al_2O_3	3.95 SiO_2		Limoges (Fine)
7	.1836 K_2O	1. Al_2O_3	3.98 SiO_2		Limoges (Common)
8	.2020 K_2O	1. Al_2O_3	4.74 SiO_2		Limoges (Figures)
9	.1336 K_2O .0961 CaO .2297 RO	1. Al_2O_3	4.66 SiO_2		Karlsbad Body
10	.2400 K_2O	1. Al_2O_3	5.2 SiO_2		American Body
11	.2450 K_2O	1. Al_2O_3	5.027 SiO_2		Limoges Body
12	.2540 K_2O	1. Al_2O_3	4.51 SiO_2		Limoges (Fine)
13	.2584 K_2O	1. Al_2O_3	5.44 SiO_2		American Body
14	.2770 K_2O	1. Al_2O_3	7.06 SiO_2		Japanese Body
15	.2700 K_2O .0100 CaO .2800 RO	1. Al_2O_3	3.95 SiO_2		Limoges Body (Common)
16	.1635 K_2O .1186 CaO .2821 RO	1. Al_2O_3	4.35 SiO_2		Karlsbad Body
17	.2860 K_2O	1. Al_2O_3	6.05 SiO_2		Berlin Body
18	.3312 K_2O	1. Al_2O_3	4.74 SiO_2		Limoges Body
19	.3330 K_2O .0200 CaO .3530 RO	1. Al_2O_3	4.72 SiO_2		Chinese Body
20	.1290 K_2O .2070 CaO .3360 RO	1. Al_2O_3	4.39 SiO_2		Sevres Body

TABLE I—Continued.

No.	Chemical Formulæ			Locality
21	.1467 K_2O .2017 CaO .3484 RO	1. Al_2O_3	4.07 SiO_2	American Porcelain (Heavy)
22	.3583 K_2O .0014 CaO .0030 PbO .3627 RO	1. Al_2O_3	3.96 SiO_2	English Porcelain
23	.1590 K_2O .2160 CaO .3750 RO	1. Al_2O_3	4.33 SiO_2	American Porcelain (Fine)
24	.2880 K_2O .1255 CaO .4135 RO	1. Al_2O_3	3.7 SiO_2	Limoges Body (Fine)
25	.1776 K_2O .2442 CaO .4218 RO	1. Al_2O_3	4.16 SiO_2	American Body (Medium)
26	.1910 K_2O .2645 CaO .4555 RO	1. Al_2O_3	3.74 SiO_2	American Body (Medium)
27	.4360 K_2O	1. Al_2O_3	4.81 SiO_2	American Body (Fine)
28	.4900 K_2O	1. Al_2O_3	4.0 SiO_2	American Body (Medium)
29	.0960 K_2O .4040 CaO .5000 RO	1. Al_2O_3	3.08 SiO_2	Sevres Body (Heavy)
30	.2240 K_2O .3010 CaO .5250 RO	1. Al_2O_3	5.34 SiO_2	Hache and Pepin (Fine)
31	.0800 K_2O 1.7560 CaO 1 8360 RO	1. Al_2O_3	3.2 SiO_2	English Bone China
32	.2580 K_2O 2.0780 CaO 2.3360 RO	1. Al_2O_3	15.2 SiO_2	Dr. Wall's Body*
33	.2690 K_2O 2.5400 CaO 2.8090 RO	1. Al_2O_3	3.39 SiO_2 1.67 P_2O_5	Modern English Body*

*Trans. A. C. S., Vol. III, Birth of English Porcelain.

TABLE I—Continued.

No.	Chemical Formulæ			Locality
34	.1370 K_2O 2.7370 CaO 2.8740 RO	1. Al_2O_3	4.25 SiO_2 1.09 P_2O_5	Bow Body*
35	.2300 K_2O 5.2000 CaO 5.4300 RO	1. Al_2O_3	7.12 SiO_2 1.5 P_2O_5	Chelsea Body*
36	2.9350 K_2O 3.9040 Na_2O 5.2770 CaO 12.1160 RO	1. Al_2O_3	31.43 SiO_2	Dr. Wall's Body*

At first sight this list seems a hopeless conglomeration, ranging to all extremes, but closer study discloses the fact that, while the RO varies from .0467 in the Japanese to 5.43 in the old English, and from .114 in modern Belgium to 1.84 in modern English ware, the proportion of SiO_2 is comparatively constant. Only in two out of thirty-six cases does the SiO_2 run below 3.7, and in only two cases of modern porcelain does it run over 6.0 SiO_2. The average of the list I find to be between 4. SiO_2 and 4 4 SiO_2, hence I took 4.2 SiO_2 as the standard.

I next chose a standard from the RO. To obtain this by the same method as pursued with the SiO_2 would prove unsatisfactory, since to take an average of the entire list would result in a RO of 1.00, while to take an average of the thirty modern porcelains would give us about 0.3 RO. The first case would give us about a cone 6 product while the second case gives us very nearly the type of an American porcelain maturing at cone 12.

I did not think a choice between such extremes could be satisfactory, so I concluded to make a series covering each extreme and a third about half way between. To this end I started with the three following types:

Series A.	1. RO,	1. Al_2O_3,	4.2 SiO_2	Cone 6 type
Series B.	.5 RO,	1. Al_2O_3,	4.2 SiO_2	Cone 9 type
Series C.	.3 RO,	1. Al_2O_3,	4.2 SiO_2	Cone 12 type

*Trans. A. C. S., Vol. III, Birth of English Porcelain.

Method of Production of the Trial Bodies.

The various porcelains I produced in the following manner:

The two extremes of each set, I weighed out and ground wet in a ball mill. The intermediate numbers I obtained by liquid blending of the extremes. After the blends were thoroughly mixed, I poured them into plaster molds to harden by loss of their water. Of each porcelain body I made the following ware:

2 brickettes made in standard cement,—brickette molds, and having a central cross section of 1 inch in the green state. These were made by the plastic process.

2 cubes of 50 grams weight made also by the plastic process.

2 tiles, 4½ inches x 4 ½ inches, in green state. These were made by the dry-press process.

The material for the dry pressed tile was prepared as follows: The dry body was thoroughly pulverized and was then dampened with water by means of a brush and the hand, thus obtaining a fairly uniform dampness. The damp clay was allowed to stand about two hours and was then put through a twenty-mesh screen and promptly pressed into the desired shape by means of a hand tile press. About 12 to 14 per cent. of water was in the clay when it was pressed.

Upon the various forms made up, I made the following tests:

Electrical resistance test, tensile test, abrasion test, absorption test and shrinkage test.

Preliminary Studies on the Electrical Test.

While no great obstacle lay in the way of the tensile, abrasion, absorption or shrinkage tests, I found a very great one confronting me, when I considered testing the electrical resisting properties of the porcelains. In the first place, I had no idea how much electrical resistance a given thickness of porcelain might display. In the second place, I could only press a tile of one size, viz: 4½ inches x 4½ inches. I saw that I must obtain, if possible, some insulating material in which to imbed the tile and thus

increase the arcing distance of the current, since with 4½ inches arcing distance, I could not use quite 55000 volts without the current arcing around the tile.

In searching for a material to use for the insulating apron referred to, I found that S. P. Thompson, in his treatise. "Electricity and Magnetism," page 310, gives the following table of specific resistances:

TABLE II.

Silver	100.
Copper	96.
Gold	74.
Iron Soft	16.
Dead	8.
German Silver	7.5
Mercury (liquid)	1.6
Glass at 200° C less than	.000,000,001
Gutta percha —— less than	.000,000,001
Selenium Annealed less than	.000,000,0025
Pure Water at 22° C less than	.000,001

The preceding table of conductivity or resistance, as you may please to call it, indicates that the last four substances: Glass, gutta percha, selenium and pure water are far better insulators than any other substances ready at hand. The idea of using Portland cement, however, suggested itself, and as it seemed plausable that this might be a good insulating material, I proceeded to try it.

To do this, I took a semi-vitrified tile, 4 x 4 x ⅜ inches, and incased it in a slab of Portland cement (Dyckerhoff). The cement was carefully tamped around the tile so that a good attachment might result. I also made several other slabs of cement, both German and American. In these I encased no tiles. All the cement slabs stood six days in water, and were then taken out and carefully dried. I then proceeded to test their resisting properties with poor results. Each and every slab showed only so much electrical resistance as a like thickness of pure air would show, thus proving that the small insulating power displayed by the cement was due to the fact that its thickness held the electrodes apart and made an arc necessary.

I next tested the tile encased in cement. I had also provided myself with the following insulating materials: One sheet window glass; one sheet plate glass; one sheet plate glass; one porcelain tea plate; one tile 4x4x⅜ inches unincased. The results of the test of the above was as follows:

Tile 4x4x⅜ inches, incased in slab of Portland cement, punctured at .... 17,000 volts.
Tile 4x4x⅜ inches, unincased, punctured ..... 20,250 "
Sheet window glass 6x6x⅛ punctured at..... 40,000 "
Porcelain Tea Plate 6 inches in diameter x ⅛, punctured at..... ..... 49,500 "
Sheet Plate Glass 6x6x¼ stood 60,000 volts and then the current arced around sheet and could not puncture it.

I would state here that the tile incased in cement and the unincased tile were both of the same composition, and as both were porous to a degree, it is to be presumed that the incased tile which had been soaked for six days still retained some moisture when tested, which accounts for its lower puncturing point than was displayed by the unincased tile. The facts brought out by this preliminary test seem about as follows:

First. That Portland cement does not possess any more insulating power than would be displayed by a like thickness of dry air.

Second. That porcelain, if not perfectly vitrified, does not possess any great insulating efficiency, even when perfectly dry.

Third. That glass ranks next and lies between semi-vitrified and vitrified porcelain.

Fourth. That thoroughly vitrified porcelain, even though only ⅛ inch thick, possesses as much insulating strength as is necessary, since 40,000 volts is the highest voltage that can be transmitted without arcing around a 4½ x 4½ inch tile.

Fifth. That plate glass possesses an especially high electrical resisting property.

We know that the composition of glasses has a great influence on their ability to resist electric currents. Also, that the temperature at which they are used is a factor.

The following data were obtained from Widemann on "Electricity," Vol. 1, page 560:

TABLE III.

HEAVY LEAD GLASS		WHITE MIRROR GLASS		GREEN BOTTLE GLASS	
Analysis		*Analysis*		*Analysis*	
K_2O.....	4.0	K_2O.....	3.67	K_2O ...	6.53
Na_2O...	—	Na_2O .	9.48	Na_2O...	8.68
PbO	67.0	CaO.....	16.00	CaO	12.55
SiO_2...	20.0	SiO_2... ..	67.80	SiO_2....	68.02
	100		96.88		
Melting point. 700°C.				Melting point, 1080°C.	
Temp.°C	Resistance.	Temp.°C	Resistance.	Temp.°C	Resistance.
216.	188600.	189.	182200.	170.5	173200.
226.	113160.	200.	109320.	180.4	86600.
234.	75440.	214.	54660.	198.8	34640.
249.	37720.	288.5	27330.	211.0	17320.
269.	18860.	247.	12754.	222.5	10392.
286.	11316.	269.	5466.	250.5	3464.
297.	7544.	299.	1822.	266.5	1732.
312.	3772.	316.5	1093.2	305.5	519.6
326.	1886.	329.	728.8	327.5	259.8
341.	943.	349.	364.4	343.0	173.2

The foregoing tables take mercury as the unit of conductivity, or as having a resistance of 1.

To return to my search for an aproning material to use with my tiles, I found that a gutta-percha plate could not be cemented to the tile for each test and then removed, and to furnish a separate sheet of gutta-percha for each tile would mean entirely too great an expense. Selenium also was out of the question owing to its cost. I concluded, however, to try sulphur, which is a very similar material to selenium, and to do this I took two bottomless flasks and by means of sulphur in the melted state I cemented these two flasks to two tiles. I also cast two large plates of sulphur with tiles imbedded in their centers. One tile was completely imbedded leaving only a hole ½ inch in diameter on each side to admit the electrode. The second tile was only imbedded on one side in the sulphur, the other side was left exposed. There was a one half inch hole on the imbedded

side of the tile to admit the eloctrode. The tiles were vitrified. I also cast a plate of sulphur 6x6x½ inches. The results of the electrical test on sulphur was as follows:

The plate containing the half imbedded slab stood about 58.000 volts and then the current arced through the sulphur. The plate containing the completely imbedded tile stood about the same, the current going around the tile through the sulphur. The tiles cemented to the flasks were not satisfactory since the current found its way between the glass and the sulphur. The slab of pure sulphur ½ inch thick stood about 13.500 volts and then punctured. On its being punctured the slab caught fire from the spark.

I felt satisfied that at least none of the materials available could be successfully used as an aproning material so I discarded the idea of aproning my tiles, and reduced the thickness of the pieces to be tested electrically to an amount which would puncture before the current would arc around the tile.

Formulæ of the Porcelain Bodies Made.

I have already given the formulæ of the three porcelains which I selected as fairly illustrating the types now in actual use. Taking each type as a basis, I made three separate sub-series of bodies as follows:

I. Maintaining RO and SiO_2 constant and varying the proportion of Al_2O_3.

II. Maintaining the Al_2O_3 to SiO_2 constant and varying the RO in kind but not in quantity.

III. Maintaining the RO and Al_2O_3 constant and varying the proportion of SiO_2.

Each sub-series consisted of a set of five porcelains. In this way, I expected to cover the ground most completely.

Following I give the detailed chemical formulæ of the various porcelains included in this investigation:

SERIES A—CONE 6 TYPE.

In Series A I use Dr. Seger's RO, viz.: $\left.\begin{matrix} .3\ K_2O \\ .7\ CaO \end{matrix}\right\}$

Sub-Series A-I.

A-I-1	.3 K_2O .7 CaO	.7 Al_2O_3	4.2 SiO_2
A-I-2	.3 K_2O .7 CaO	.8 Al_2O_3	4.2 SiO_2
A-I-3	.3 K_2O .7 CaO	.9 Al_2O_3	4.2 SiO_2
A-I-4	.3 K_2O .7 CaO	1.0 Al_2O_3	4.2 SiO_2
A-I-5	.3 K_2O .7 CaO	1.1 Al_2O_3	4.2 SiO_2

*Series A-II.**

A-II-1	.3 K_2O .7 CaO	1. Al_2O_3	4.2 SiO_2
A-II-2	.4 K_2O .6 CaO	1. Al_2O_3	4.35 SiO_2
A-II-3	.5 K_2O .5 CaO	1. Al_2O_3	4.5 SiO_2
A-II-4	.6 K_2O .4 CaO	1. Al_2O_3	4.65 SiO_2
A-II-5	.7 K_2O .3 CaO	1. Al_2O_3	4.8 SiO_2

Series A-III.

A-III-1	.3 K_2O .7 CaO	1. Al_2O_3	3.2 SiO_2
A-III-2	.3 K_2O .7 CaO	1. Al_2O_3	4.2 SiO_2
A-III-3	.3 K_2O .7 CaO	1. Al_2O_3	5.2 SiO_2
A-III-4	.3 K_2O .7 CaO	1. Al_2O_3	6.2 SiO_2
A-III-5	.3 K_2O .7 CaO	1. Al_2O_3	7.2 SiO_2

SERIES B — CONE 9 TYPE.

Series B-I.

B-I-1	.3 K_2O .2 CaO	.7 Al_2O_3	4.2 SiO_2
B-I-2	.3 K_2O .2 CaO	.8 Al_2O_3	4.2 SiO_2

* In Series A-II it was necessary to vary the SiO_2 content slightly in order to introduce the additional K_2O as feldspar.

B-I-3	.3 K_2O .2 CaO	.9 Al_2O_3	4.2 SiO_2
B-I-4	.3 K_2O .2 CaO	1.0 Al_2O_3	4.2 SiO_2
B-I-5	.3 K_2O .2 CaO	1.1 Al_2O_3	4.2 SiO_2

Series B-II.

B-II-1	.5 CaO	1. Al_2O_3	4.2 SiO_2
B-II-2	.375 CaO .125 K_2O	1. Al_2O_3	4.2 SiO_2
B-II-3	.25 CaO .25 K_2O	1. Al_2O_3	4.2 SiO_2
B-II-4	.125 CaO .375 K_2O	1. Al_2O_3	4.2 SiO_2
B-II-5	.5 K_2O	1. Al_2O_3	4.2 SiO_2

Series B-III.

B-III-1	.3 K_2O .2 CaO	1. Al_2O_3	3.2 SiO_2
B-III-2	.3 K_2O .2 CaO	1. Al_2O_3	4.2 SiO_2
B-III-3	.3 K_2O .2 CaO	1. Al_2O_3	5.2 SiO_2
B-III-4	.3 K_2O .2 CaO	1. Al_2O_3	6.2 SiO_2
B-III-5	.3 K_2O .2 CaO	1. Al_2O_3	7.2 SiO_2

SERIES C—CONE 12 TYPE.

Series C-I.

C-I-1	.2 K_2O .1 CaO	.7 Al_2O_3	4.2 SiO_2
C-I-2	.2 K_2O .1 CaO	.8 Al_2O_3	4.2 SiO_2
C-I-3	.2 K_2O .1 CaO	.9 Al_2O_3	4.2 SiO_2
C-I-4	.2 K_2O .1 CaO	1.0 Al_2O_3	4.2 SiO_2
C-I-5	.2 K_2O .1 CaO	1.1 Al_2O_3	4.2 SiO_2

Series C-II.

C-II-1	.3 CaO	1. Al_2O_3 4.2 SiO_2
C-II-2	.225 CaO .075 K_2O	1. Al_2O_3 4.2 SiO_2
C-II-3	.15 CaO .15 K_2O	1. Al_2O_3 4.2 SiO_2
C-II-4	.075 CaO .225 K_2O	1. Al_2O_3 4.2 SiO_2
C-II-5	.3 K_2O	1. Al_2O_3 4.2 SiO_2

NOTE—Owing to a lack of time, no attempt was made to run a C-III Series, but an approximate idea on the variation of silica at this temperature will be obtained by a study of Series C-I.

PRELIMINARY TEST.

I was unwilling to spend the great amount of time and labor on this investigation, without some assurance that the mixtures I proposed to make, as shown by the preceding formulæ, would give me the range of workable porcelains which I expected. I therefore made cones of all the various porcelain bodies to be tested and each series was tested against a standard Seger cone of the temperature to which that series was to be fired. The results of this test were as follows:

Series A—tested against Cone 6.

Specimen A-I-1 shows about the same fusing point as cone 5, hence is useless at this temperature.

Specimen A-I-2 developed into a splendid porcelain of high grade. Translucency is at its maximum, but indications of fusion are apparent.

Specimen A-I-3 is a splendid porcelain with a slight trace of stoniness and good translucency.

Specimen A-I-4 gives a perfectly stony porcelain, but perfectly vitrified and with slight translucency.

Specimen A-I-5 evidently too high in Al_2O_3 for this temperature. Has a dead white surface with no translucency and evidence of porosity.

Specimen A-II-1 is the same as A-I-3 and shows a very similar development.

Specimen A-II-2 is somewhat further developed than A-II-1, but only shows translucency on the edges.

Specimen A-II-3 begins to take on a characteristic porcelain appearance, quite translucent with slight stoniness.

Specimen A-II-4 has about the same degree of development as displayed in A-I-3, good translucency with only slight stoniness.

Specimen A-II-5. We here reach again the high grade porcelain type, all traces of stoniness having disappeared and the surface being self-glazed, as in A-I-2.

Specimen A-III-1 shows a hard dead white surface with no signs of translucency and with indications of porosity.

Specimen A-III-2 same as A-I-4 and A-II-1.

Specimen A-III-3. Slightly improved over A-III-2 in translucency but otherwise unchanged.

Specimen A-III-4 shows fair translucency but is too high grade a porcelain for much strength.

Specimen A-III-5. Good translucency but is semi-glazed, indicating over maturity.

Conclusions on Appearance of Series A.

Series A-I seems to indicate that at this temperature we may expect good porcelains with from .8 to 1.0 Al_2O_3 Below .8 Al_2O_3, we find the body melts out of shape and above 1. Al_2O_3 it does not mature. .8 or .9 Al_2O_3 gives a fine translucent porcelain at cone 6.

Series A-II shows that CaO alone does not produce a porcelain and apparently does not flux materially at this temperature. Evidently, CaO does not produce any translucency, but changes suddenly from the dry to the glassy state. K_2O seems to have done its maximum amount of fluxing, since A-II-5 shows a splendid degree of vitrification and translucency.

Series A-III seems to indicate that an increase of SiO_2 does not affect the appearance of the porcelain to the same degree as do the other two constituents. However, it shows that to obtain a good porcelain, the SiO_2 should be between 4.2 and 6.2 SiO_2, as below 4.2 SiO_2 we get immaturity and above 6.2 SiO_2 we get a weak glassy product.

Series B. Tested Against Cone 9.

Specimen B-I-1 shows about the same degree of development as cone 9, so that its absolute fusing point is too nearly reached to make it a practical porcelain at this temperature.

Specimen B-I-2 is a brilliant, self-glazed porcelain, and has a fine translucence.

Specimen B-I-3 shows a fair degree of translucency and a tendency to stoniness.

Specimen B-I-4 is a stony appearing porcelain with only traces of translucency on edges.

Specimen B-I-5 is a very stony body with a little or no trace of translucency.

Specimen B-II-1 could not properly be called a porcelain. It is a porous, cream-white biscuit,

Specimen B-II-2 displays a marked improvement over B-II-1 in that it is a closer, stonier body, but signs of porosity are still noted.

Specimen B-II-3 seems to be a good stony porcelain, and from all appearances is perfectly vitrified. It displays traces of translucency on all exposed points and has a smooth satiny surface.

Specimen B-II-4 is a fine porcelain, with good translucency and a smooth surface. It still retains some signs of stoniness.

Specimen B-II-5 gives the finest porcelain so far obtained. Perfect translucency without a trace of stoniness and brilliantly self-glazed on all exposed points.

Specimen B-III-1 shows a slightly undermatured porcelain but does not appear porous.

Specimen B-III-2. Same as B-I-4, and shows a similar development.

Specimen B-III-3 displays a fair translucency with traces of stoniness.

Specimen B-III-4 is a high grade porcelain with maximum translucency and a self-glazed surface.

Specimen B-III-5 shows signs of overmaturity. It has an opaque glassy appearance on the surface which destroys translucency.

Conclusions on Appearance of Series B.

Series B-I seems to indicate that we may expect good porcelain with .8 to 1.0 Al_2O_3, the RO and SiO_2 being as shown in this series.

Series B-II appears to show that we may not expect to obtain porcelain at this temperature unless we use at least one-half the RO as alkali. Above this amount we get good porcelains in every case, even where we use K_2O alone.

Series B-III confirms the conclusion in A-III that the variation of SiO_2 does not vary the appearance of the product so much as a variation of either of the other constituents. However, 3.2 SiO_2 seems insufficient, while 7.2 SiO_2 in undoubtedly an excess.

Series C. Tested Against Cone 12.

Specimen C-I-1 shows a maximum maturity. Very translucent and brilliantly self-glazed.

Specimen C-I-2 is a splendid porcelain, fine translucency and a good smooth surface.

Specimen C-I-3 shows a fair degree of translucency, with traces of stoniness.

Specimen C-I-4. Slightly translucent, marked stoniness.

Specimen C-I-5. Somewhat undermatured and little, if any, translucency.

Specimen C-II-1 is a good hard biscuit, but without any signs of translucency.

Specimen C-II-2 is a hard body, but still lacks any traces of translucency.

Specimen C-II-3 gives a good stony porcelain, with traces of translucency on exposed points.

Specimen C-II-4 is a fine translucent porcelain.

Specimen C-II-5 shows a very strongly translucent porcelain with an extremely glassy surface.

Conclusions on Appearance of Series C.

Series C-I shows us that undoubtedly a good porcelain at this temperature must contain, at least 0.8 Al_2O_3 and not over 1.0 Al_2O_3.

Series C-II indicates that even at this temperature we get no translucency from CaO. With all the RO as K_2O, we get a fine appearing body with maximum translucency.

We conclude from the previous tests and from Series C-I that Series C-III would give us a graded increase from stoniness to glassiness.

PREPARATION OF THE REGULAR TRIAL PIECES.

The preliminary testing having been completed, and having reassured me of the suitability of the series as projected to bring out the facts for which we were seeking, I then began the main work. The various bodies were prepared on sufficient scale, and were worked into the shapes heretofore decided upon and after drying each lot thoroughly, I fired them. It was impossible to obtain in the small kiln used, an absolutely uniform temperature over the entire kiln. However, I think I can say, that except in one case, all received an equal period of firing. All the pieces were imbedded in silica sand, except Series A-III from which the sand leaked away to some extent during the firing process. Series A had to be refired, as it was not fully matured at the first firing. Of course in such a line of work as I have pursued, the factor of error due to a variation of the process is very great, but I have made an effort to work all the bodies as nearly alike as possible and think that even with these factors of error present, we may find some valuable facts by a study of the results of the tests.

REMARKS ON THE RESULTS OF SERIES A.

ELECTRICAL RESISTANCE

As I only had 4½"x4½" tile for this test, I could not expect to obtain above a test of 50,000 volts. For this reason I reduced the thickness of my tile at the center to about $\frac{3}{16}$ inch. The voltage for this test was obtained by means of a transformer provided for me by the Electrical Engineering Department of Ohio State University and operated by R. D. DeWolf, fellow in that department. Our power was obtained from the power circuit of O. S. U.

The test resulted as follows:

Series A-I—See Table

A-I-1 was melted out of shape and could not be tested. A-I-2 and A-I-3 stood the maximum voltage, arcing at 47,250 volts without puncturing. A-I-4 and A-I-5 both show a decreasing efficiency as insulators.

Of this group, I think A-I-3 seems the most desirable.

Series A-II—See Table

This series gives apparent confirmation to my opinion that a glassy porcelain has not the insulating strength of a semi-stony one.

A-II-1 shows a poor insulating power, due to imperfect vitrification.

A-II-2 seems improved in vitrification and shows a higher resistance.

A-II-3, which proved the best in the tensile test, again seems preferable, standing the maximum voltage applied.

A-II-4 stands the same voltage without puncturing, as did A-II-3, but its inferiority as shown in the tensile test makes it less desirable than the preceding specimen.

A-II-5 failed to stand up under the voltage applied and punctured at a maximum of 36,900 volts.

This, with its indicated weakness in tensile strength, would lead us to believe that we must look elsewhere than to pure feldspar porcelains for the highest insulating materials.

Series A-III—See Table

This series shows an increasing electrical resistence with an increasing proportion of SiO_2.

A-III-1 could not be worked in a dry press process due to the high clay contents. All the tiles cracked in burning, hence no electrical test was obtained.

A-III-2 punctured at 13,050 and 10,250 volts, respectively.

A-III-3 was only tested in $\frac{1}{2}$ inch thickness, as all the $\frac{3}{16}$ inch specimens were imperfect. At $\frac{1}{2}$ inch thickness, this specimen stood 39,400 volts without puncturing, the current arcing around the tile at this voltage.

A-III-4 at $\frac{3}{16}$ inch thickness stood the maximum voltage without puncturing and arced at 49,500 volts. A $\frac{3}{32}$ inch tile punctured at 38,250 volts.

A-III-5 at $\frac{3}{16}$ inch thick could not be punctured but arced around at 49,500 volts. However, the specimens all show over-maturity, which makes this composition an undesirable one.

I should consider A-III-4 as the best of this series.

ABRASION TEST.

The abrasion test was made by filling a large ball mill with two hundred pounds of flint pebbles and enough water to cover them completely. The brickettes were then weighed and placed in the mill, which was then closed and started. Two separate sets of tests were made upon each type of porcelain. One set was subjected to a test of 1000 revolutions and the other was subjected to a 2000 revolution test. The results are as follows:

Series A-I—See Table

A-I-1 seems to have either been softened and weakened by over-maturity, or to be naturally of a weak, fragile composition, due to lack of Al_2O_3 in its composition. The specimens of this type both wore badly, although they did not chip.

A-I-2 seems to have stood the test remarkably in one case, and broken down badly in the other case.

A-I-3 stood the 1000 revolution test and showed no signs of wear. It wore badly in the 2000 revolution test, however.

A-I-4 stood the 1000 revolution test well, but lost 2.03 per cent. on the 2000 revolution test.

A-I-5 lost 1.02 per cent. in the 1000 revolution test, indicating a uniform density and maturity throughout.

From the evidence at hand, A-I-4 is undoubtedly the choice of this series.

Series A-II—See Table

A-II-1, 2 and 3 can not be very satisfactorily compared from the data obtained, since one of each type was broken in the test and lost heavily from chipping.

A-II-4 and 5, both stood the tests without breaking in either case, but both lost considerably by wear.

Of this, Series A-II-4 seems the most desirable.

Series A-III—See Table

A-III-1 wore badly in both cases.

A-III-2 and 3 stood perfectly in the 1000 revolution test, but both broke in the 2000 revolution test.

A-III-5 loses nothing in either test. Hence, we must accept A-III-5 as the best as indicated from this test.

ABSORPTION TEST.

We only find three specimens of this series that are not completely vitrified. These are, A-I-5 and A-III-1 and 2. I cannot explain the cause of A-III-2 being soft. I think some error has been made in compounding this specimen, since A-III-2 seems at fault whenever tested, while A-I-2 and A-II-1, which are of the same composition, show no lack of vitrification.

TENSILE STRENGTH.

This test was made on a standard machine, using pasteboard around the ends of the brickettes to act as cushions and prevent cracking from direct contact with the jaws of the machine which were plain and not fitted with rolls as are the jaws of machines now made for testing cement brickettes. The brickettes when made had a central cross section of one square inch. This was reduced by shrinkage in drying and firing so that it was necessary to calculate the tensile strength per square inch in each case. The fractures resulting from this test proved very interesting and I believe very instructive. They are as follows:

Series A-I—See Tables

A-I-1 shows a glassy fracture, somewhat bloated and very open.

A-I-2 shows a very glassy fracture, but not so badly bloated as A-I-1.

A-I-3 has a fairly dense semi-glassy plane of fracture with little or no signs of bloating.

A-I-4 shows a dense porcelain fracture, somewhat stony in appearance.

A-I-5 has a dull grey fracture plane which shows traces of immaturity and porosity. A-I-5*a* showed a weak center which was yellow and apparently under-matured.

Of Series A-I, the strongest is either A-I-3 or A-I-4. A-I-3 shows considerable strength, but the bloating indicated over-maturity. A-I-5 shows weakness from immaturity, as might be expected.

Series A-II—See Tables

A-II-1 being the same as A-I-4 in composition, has much the same general appearance.

A-II-2 shows a slight increase in glossiness and a smoother fracture.

A-II-3 has a glossy fracture but is very dense. A-II-3b has large interior flaw which weakens it.

A-II-4 has a very glossy fracture which discloses minute cavities as evidence that bloating has begun.

A-II-5 displays a very badly bloated fracture full of large cavities.

Of the A-II series the test shows A-II-3 to be the best. This specimen has a composition of

$$\left.\begin{matrix}5\ K_2O\\5\ CaO\end{matrix}\right\}\ 1.Al_2O_3\left\{4.2\ SiO_2\right.$$

which seems to indicate that where calcium is much in excess, we do not get the degree of density unless it be just on the verge of the fusing period. Without density we cannot have strength. At the same time the test seems to condemn A-I-5 as being brittle from excess of K_2O which caused glassiness.

Series A-III—See Table

A-III-1 shows an under-matured creamy fracture with evidence of considerable porosity. A-III-1a has a flaw in the line of fracture, hence its weakness.

A-III-2 shows a somewhat denser stonier fracture and but little sign of glassiness. A-III-2a has a flaw along its plane of fracture.

A-III-3 does not show a very much denser structure than does A-III-2. I can not explain this.

B-II-3 loses nothing in the 1000 revolution test.

B-II-4 shows its increasing glassiness, due to its increasing K_2O content. It breaks in the 2000 revolution test and loses 10 per cent.

B-II-5 loses 10 per cent. in the 1000 revolution test and over 12 per cent. in the 2000 revolution test.

I think we are safe in selecting B-II-3 as the choice of this series.

Series B-III—See Table

B-III-1 loses about the average one per cent. in the 1000 revolution test and two per cent. in the 2000 revolution test.

B-III-2 broke in the 1000 test and lost 4.4 per cent. In the 2000 test it loses only 2.27 per cent.

B-III-3 broke in the 2000 revolution test, but stood the best test of any in the 1000 revolution test.

B-III-4 lost three per cent. and B-III-5 lost 3.45 per cent. by actual abrasion in the 2000 revolution test.

I think we may safely select B-III-3 as the best of this series.

ABSORPTION TEST.

The absorption test on series B shows only one specimen to be at all porous, viz.: B-II-1, in which CaO alone was used as a flux.

TENSILE TEST.

Series B-I—See Table

B-I-1 shows a porous, glassy fracture.

B-I-2 shows a much denser, but still a glassy fracture.

B-I-3 shows some signs of stoniness with a dense dull fracture.

B-I-4 shows a decidedly stony fracture, with a flinty appearing surface.

B-I-5 displays increased stoniness.

The tensile tests favor specimen B-I-4 very much, and since it stands 2449 pounds per square inch, in one specimen having an internal flaw, I think we may say that from this test we choose B-I-4 as the best.

Series B-II—See Table

In B-II series we see again, as in series A-II, the gradation caused by replacement of CaO by K_2O as a flux.

B-II-1 shows only a dense biscuit-like body with slight porosity.

B-II-2 has lost its porosity, but still lacks any trace of translucency and displays a purely clayey fracture.

B-II-3 give a splendid stony porcelain, with a dense smooth plane of fracture.

B-II-4 shows a glassier fracture plane and an appearance of brittleness.

B-II-5 has a brilliant fracture surface and indicates that it is decidedly brittle.

In this series the test shows very clearly that when more than 50 per cent. of the RO is K_2O, the body begins to deteriorate from brittleness, while with over 50 per cent. of the RO in the form of CaO, we note immaturity or lack of vitrification.

Series B-III—See Table

Series B-III shows an increasing strength with an increasing silica content.

B-III-1 shows a dull, stony plane of fracture.

B-III-2 shows an improvement in the appearance of the fracture over that noted in the preceding.

B-III-3 shows a very fair porcelain-like break with slight glassiness.

B-III-4 shows the same break as noted in fine high grade porcelains, but somewhat less translucency is apparent.

B-III-5 shows some signs of open structure due to overmaturity.

Of this, Series B-III-4 seems the most promising, with B-III-3 a close second.

SHRINKAGE TEST.

The shrinkage test on series B seemed to show little that we did not already know. The specimens high in K_2O maintain their size uniform with the rest of the series. Where two specimens of the same composition are unequally

matured, the specimen showing the greatest shrinkage invariably stands the severest test.

I think we may safely select B-I-2 from the B-I series as showing the best average in all tests. In B-II series, we find B-II-3 leads the others. In B-III series, specimen B-III-3 is the favorite.

REMARKS ON THE RESULTS OF SERIES C.

ELECTRICAL RESISTANCE.

Note that in series C the plates were only ⅛ inch in thickness, instead of 3-16 inch as in previous tests. A test is also recorded at ¼ inch thick.

C-I-1 stands the maximum, 34,000 volts at ⅛ inch thickness, and is not punctured at 47,500 at ¼ inch thick.

C-I-2 and 3 show a decreasing efficiency as insulators. C-I-4 is notably better than the two preceding in this respect, perhaps owing to its highly developed stony structure. The choice in this series is, however, C-I-1 or 2.

Series C-II shows an increasing electrical resistance with an increasing K_2O contents, but every specimen tested punctured, the best being the one of pure K_2O flux. This is contrary to the evident influences of K_2O alone as a flux according to the observations at lower temperatures. We must concede C-II-4 and 5 as the best of this series from the insulating efficiency evinced in this test.

ABRASION TEST.

Series C-I shows a magnificent record in this test. Both the 1000 and the 2000 revolution tests failed to in any way affect any of this series, and the blocks tested showed no evidence of having undergone any abrasive action whatever. I take it that this is a very strong argument in favor of high fire porcelain.

C-II-1 with CaO alone as a flux loses 8 to 12 per cent. by pure abrasion, showing that it lacks density and strength.

C-II-2 loses only 1.63 per cent. in the worst specimen, showing a marked improvement.

C-II-3 and 4 lose nothing in either case.

C-II-5 with K_2O alone as a flux loses 3.33 per cent. and 2.34 per cent. by chipping, which seems a characteristic of pure potash porcelain.

Of C-II series we may safely say that favorite is either C-II-3 or 4.

ABSORPTION TEST.

The test showed the entire series to be completely vitrified.

On the series C-II tests, specimen C-II-1 and 2 showed lack of perfect vitrification, the other members all proving perfectly vitrified.

TENSILE TEST.

Series C-I—See Table

C-I-1 shows a glassy fracture with a slight trace of over-maturity. That it is at its best is evinced by one specimen standing 2,100 pounds per square inch.

C-I-2 shows a fine close body with a good bright plane of fracture but no signs of over-maturity. One specimen stood 3071 pounds per square inch, the maximum strength displayed by any specimen in this work.

C-I-3 shows a good close structure and a tendency to stoniness.

C-I-4 is an extremely dense but a very stony porcelain.

C-I-5 shows an excess of clay in its composition as it has a dead and weak appearance to its fracture.

C-I-1 and 2 show the highest degree of tensile development of this series.

Series C-II—See Table

C-II-1 gives us only a good biscuit.

C-II-2 is a very hard biscuit, but lacking in vitrification and translucency.

C-II-3 has a close structure with good vitrification and dense stoniness.

C-II-4 is a good semi-stony porcelain with a splendid appearance of translucency.

C-II-5 is a fine well matured porcelain of a glassy nature.

The choice of the above as indicated by the tensile test would be C-II-4 or 5.

SHRINKAGE TEST.

The tests showed a slightly increasing shrinkage with an increase of Al_2O_3 from clay substance until C-I-3 is reached. Above this no increased shrinkage is apparent. In the C-II series, the increased K_2O seemed to cause increased shrinkage.

C-II-2 for some reason shrinks equally with C-II-4 and 5. My only explanation of this would be that the large amount of clay substance present in this specimen is enabled to act by the addition of the small amount of feldspar.

Why C-II-3, also, did not shrink proportionately I can not explain. I note, however, that in every case the first addition of K_2O seems to cause an increased shrinkage, not continuing to increase in the higher K_2O porcelains.

We may choose from Series C-I as favorites, either C-I-1 or 2. From Series C-II, I think C-II-4 is the best.

The data which have been discussed in the foregoing, will be found in tabular form in the following eight pages:

TESTS OF SERIES.

A—I

ELECTRICAL RESISTANCE TEST.

No.		Thickness	Puncturing Voltage	Remarks:
1	a	———	———	Melted out of shape and could not be tested.
	b	———	———	
2	a	$\frac{3}{16}$ inch.	Not punct'd	Arced around at 47,250 Volts.
	b	$\frac{3}{16}$ "	" "	" " at 47,250 "
3	a	$\frac{3}{16}$ "	" "	" " at 47,250 "
	b	—	—— ——	
4	a	$\frac{3}{16}$ "	14,400	
	b	$\frac{3}{16}$ "	13,500	
5	a	$\frac{3}{16}$ "	11,475	
	b	$\frac{3}{16}$ "	10,575	

ABRASION TEST
With 200 Pounds Flint Pebbles in Wet Ball-Mill

No.		Before Test	After Test	Per Cent. Loss
1	a	51.0 grs.	49.0 grs.	3.92
	b	49.0 "	47.0 "	4.08
2	a	52.5 "	52.0 "	0 95
	b	63.0 "	58.5 "	7.14
3	a	48.0 "	47.0 "	2.08
	b	48.7 "	48.5 "	0.51
4	a	49.2 "	48.2 "	2.03
	b	47.5 "	47.5 "	——
5	a	48.0 "	47.0 "	2.08
	b	49.0 "	48.5 "	1.02

a= 2000 revolution test.
b= 1000 " "

ABSORPTION TEST.

No.		Before Soaking	After Soaking	Per Cent. Absorpt'n
1	a	51.0 grs.	51.0 grs.	——
	b	49.0 "	49.0 "	——
2	a	52.5 "	52.0 "	——
	b	63.0 "	63.0 "	——
3	a	48.0 "	48.0 "	——
	b	48.7 "	48.7 "	——
4	a	49.2 "	49.2 "	——
	b	47.5 "	47.5 "	——
5	a	45.5 "	48.0 "	5.50
	b	48.5 "	49.0 "	1.03

TENSILE TEST.

No.		Area of Cross Section.	Load Neces'ary to Break	Tensile Strength of 1 Sq. Inch
1	a	$\frac{13}{16}$ x $\frac{14}{16}$	912 lbs.	1390 lbs.
	b	— x —	—— "	—— "
2	a	$\frac{17}{16}$ x $\frac{14}{16}$	1777 "	2408 "
	b	$\frac{17}{16}$ x $\frac{12}{16}$	1653 "	2161 "
3	a	$\frac{14}{16}$ x $\frac{12}{16}$	1882 "	2373 "
	b	$\frac{14}{16}$ x $\frac{14}{16}$	1918 "	2505 "
4	a	$\frac{14}{16}$ x $\frac{17}{16}$	1766 "	2391 "
	b	$\frac{14}{16}$ x $\frac{12}{16}$	2038 "	2866 "
5	a	$\frac{14}{16}$ x $\frac{14}{16}$	904 "	1181 "
	b	$\frac{14}{16}$ x $\frac{14}{16}$	1566 "	2056 "

SHRINKAGE TEST.

No.		Area Green	Area Burned	Per Cent. Loss
1	a	1. Sq. In.	.656 sq. in	34.40
	b	— "	—— "	——
2	a	1. "	.738 "	26.20
	b	1. "	.764 "	23.54
3	a	1. "	.793 "	20.70
	b	1. "	.765 "	23.44
4	a	1. "	.738 "	26.17
	b	1. "	.711 "	28.90
5	a	1. "	.765 "	23.44
	b	1. "	.761 "	23.83

TESTS OF SERIES. (Continued)

A-II.

ELECTRICAL RESISTANCE TEST.

No.		Thickness	Puncturing Voltage	Remarks:
1	a	$\frac{8}{16}$ inch	10,700	
	b	— "	—	
2	a	$\frac{8}{16}$ "	23,625	
	b	$\frac{8}{16}$ "	36,000	
3	a	$\frac{8}{16}$ "	Not punct'd	Arced around at 47,250 Volts
	b	$\frac{8}{16}$ "	" "	" " " 47,250 "
4	a	$\frac{8}{16}$ "	" "	" " " 47,250 "
	b	$\frac{8}{16}$ "	" "	" " " 47,250 "
5	a	$\frac{8}{16}$ "	36,900	
	b	$\frac{8}{16}$ "	27,450	

ABRASION TEST
With 200 Pounds Pebbles in Wet Ball-Mill

No.		Before Test	After Test	Per Cent. Loss
1	a	40.0 grs.	38.0 grs.	5.00
	b	45.0 "	43.0 "	4.44
2	a	45.0 "	43.0 "	4.44
	b	45.2 "	44.5 "	1.67
3	a	49.5 "	46.5 "	6.05
	b	49.0 "	48.5 "	1.02
4	a	53.5 "	53.0 "	0.94
	b	45.5 "	45.0 "	1.10
5	a	55.0 "	54.0 "	1.81
	b	50.5 "	50.0 "	0.99

a = 2000 revolution test.
b = 1000 " "

ABSORPTION TEST.

No.		Before Soaking	After Soaking	Per Cent. Absorpt'n
1	a	40.0 grs.	40.0 grs.	—
	b	45.0 "	45.0 "	—
2	a	45.0 "	45.0 "	—
	b	45.2 "	45 2 "	—
3	a	49.5 "	49.5 "	—
	b	49.0 "	49.0 "	—
4	a	53.5 "	53.5 "	—
	b	45.5 "	45.5 "	—
5	a	55.0 "	55.0 "	—
	b	50.5 "	50.5 "	—

TENSILE TEST.

No.		Area of Cross Section	Load Neces'ary to Break	Tensile Strength of 1 Sq. Inch
1	a	$\frac{12}{16} \times \frac{14}{16}$	887 lbs.	1118 lbs.
	b	$\frac{14}{16} \times \frac{14}{16}$	1416 "	1850 "
2	a	$\frac{16}{16} \times \frac{14}{16}$	1382 "	2023 "
	b	$\frac{17}{16} \times \frac{14}{16}$	1434 "	2092 "
3	a	$\frac{17}{16} \times \frac{14}{16}$	1743 "	2543 "
	b	$\frac{21}{16} \times \frac{14}{16}$	*1439 "	2099 " *
4	a	$\frac{16}{16} \times \frac{22}{16}$	1190 "	1681 "
	b	$\frac{16}{16} \times \frac{22}{16}$	1175 "	1660 "
5	a	$\frac{18}{16} \times \frac{22}{16}$	1358 "	1845 "
	b	$\frac{18}{16} \times \frac{15}{16}$	*525 "	690 " *

* Indicates a defective brickette.

SHRINKAGE TEST.

No.		Area Green	Area Burned	Per Cent. Loss
1	a	1. Sq. In.	.793 sq.in	20.70
	b	1. "	.765 "	23.50
2	a	1. "	.683 "	31.65
	b	1. "	.685 "	31.45
3	a	1. "	.685 "	31.45
	b	1. "	.685 "	31.45
4	a	1. "	.708 "	29.20
	b	1. "	.708 "	29.20
5	a	1. "	.736 "	26.40
	b	1. "	.761 "	23.83

TESTS OF SERIES. (Continued.)

A-III

ELECTRICAL RESISTANCE TEST.

No.		Thickness	Puncturing Voltage	Remarks:
1	a	$\frac{3}{16}$ inch.	————	Broke in burning.
	b	————	————	
2	a	$\frac{3}{16}$ "	18,050	
	b	$\frac{3}{16}$ "	10,250	
3	a	$\frac{3}{16}$ "	19,200	
	b	$\frac{3}{16}$ "	20,750	
4	a	$\frac{3}{16}$ "	38,250	
	b	$\frac{3}{16}$ "	Not punct'd	Arced around at 49,500 Volts.
5	a	$\frac{3}{16}$ "	" "	" " at 49,500 "
	b	$\frac{3}{16}$ "	————	Defective.

ABRASION TEST
With 200 Pounds Flint Pebbles in Wet Ball-Mill

No.		Before Test	After Test	Per Cent. Loss
1	a	38.5 grs.	37.2 grs.	3.25
	b	50.2 "	49.0 "	2.49
2	a	42.0 "	40.0 "	4.76
	b	45.2 "	45.2 "	——
3	a	45.5 "	43.5 "	4.40
	b	42.0 "	42.0 "	——
4	a	53.0 "	52.2 "	1.41
	b	47.0 "	47.0 "	——
5	a	45.5 "	45.5 "	——
	b	50.0 "	50.0 "	——

a=2000 revolution test.
b=1000 " "

ABSORPTION TEST.

No.		Before Soaking	After Soaking	Per Cent. Absorpt'n
1	a	33.5 grs.	38.5 grs.	13.16
	b	44.5 "	50.2 "	12.92
2	a	40.5 "	42.0 "	3.70
	b	43.0 "	45.2 "	5.23
3	a	45.5 "	45.5 "	——
	b	42.0 "	42.0 "	——
4	a	53.0 "	53.0 "	——
	b	47.0 "	47.0 "	——
5	a	45.5 "	45.5 "	——
	b	50.0 "	50.0 "	——

TENSILE TEST.

No.		Area of Cross Section.	Load Neces'ary to Break	Tensile Strength of 1 Sq. Inch
1	a	$\frac{29}{32} \times \frac{27}{32}$	380 lbs.	498 lbs.*
	b	$\frac{29}{32} \times \frac{29}{32}$	1035 "	1260 "
2	a	$\frac{27}{32} \times \frac{25}{32}$	698 "	1059 " *
	b	$\frac{27}{32} \times \frac{25}{32}$	1200 "	1820 "
3	a	$\frac{29}{32} \times \frac{13}{16}$	1288 "	1749 "
	b	$\frac{29}{32} \times \frac{27}{16}$	1215 "	1624 "
4	a	$\frac{27}{32} \times \frac{25}{32}$	1403 "	2130 "
	b	$\frac{13}{16} \times \frac{13}{16}$	1724 "	2612 "
5	a	$\frac{25}{32} \times \frac{13}{16}$	1985 "	2902 "
	b	$\frac{25}{32} \times \frac{13}{16}$	1567 "	2292 "

*Indicates a defective brickette.

SHRINKAGE TEST.

No.		Area Green	Area Burned	Per Cent. Loss
1	a	1. Sq. In.	.764 sq. in	23.54
	b	1. "	.821 "	17.88
2	a	1. "	.659 "	34.08
	b	1. "	.659 "	34.08
3	a	1. "	.736 "	26.37
	b	I. "	.748 "	25.20
4	a	1. "	.659 "	34.08
	b	1. "	.660 "	33.99
5	a	1. "	.683 "	31.64
	b	1. "	.683 "	31.64

TESTS OF SERIES. (Continued)

B-I.

ELECTRICAL RESISTANCE TEST.

No.		Thickness	Puncturing Voltage	Remarks:
1	a	$\frac{3}{16}$ inch	Not punct'd	Arced around at 47,850 Volts.
	b	$\frac{3}{16}$ "	" "	" " " 47,850 "
2	a	$\frac{3}{16}$ "	" "	" " " 47,850 "
	b	$\frac{3}{16}$ "	" "	" " " 47,850 "
3	a	$\frac{3}{16}$ "	17,100	
	b	$\frac{3}{16}$ "	16,200	
4	a	$\frac{3}{16}$ "	18,000	
	b	— "	—	
5	a	$\frac{3}{16}$ "	26,550	
	b	$\frac{3}{16}$ "	27,450	

ABRASION TEST
With 200 Pounds Flint Pebbles in Wet Ball-Mill

No.		Before Test	After Test	Per Cent. Loss
1	a	45.5 grs.	45.0 grs.	1.10
	b	42.5 "	42.0 "	1.17
2	a	41.0 "	40.0 "	1.22
	b	50 5 "	50.0 "	0.99
3	a	42.7 "	42.0 "	1.75
	b	41.5 "	41.0 "	1.20
4	a	43.7 "	43.0 "	1.71
	b	41.5 "	40.5 "	2.16
5	a	42.7 "	40.5 "	5.26
	b	38.5 "	38.2 "	0.64

a=2000 revolution test.
b=1000 " "

ABSORPTION TEST.

No.		Before Soaking	After Soaking	Per Cent. Absorpt'n
1	a	47.7 grs.	47.7 grs.	—
	b	42.5 "	42.5 "	—
2	a	51.0 "	51.0 "	—
	b	50.5 "	50.5 "	—
3	a	44.0 "	44.0 "	—
	b	41.5 "	41.5 "	—
4	a	45.7 "	45.7 "	—
	b	41.5 "	41.5 "	—
5	a	46.0 "	46.0 "	—
	b	38.5 "	38.5 "	—

TENSILE TEST.

No.		Area of Cross Section	Load Neces'ary to Break	Tensile Strength of 1 Sq. Inch
1	a	$\frac{29}{32} \times \frac{13}{16}$	1455 lbs.	1977 lbs.
	b	$\frac{15}{16} \times \frac{14}{16}$	1339 "	1959 "
2	a	$\frac{19}{32} \times \frac{13}{16}$	990 "	1345 "
	b	$\frac{14}{16} \times \frac{13}{16}$	1514 "	2129 "
3	a	$\frac{25}{32} \times \frac{14}{16}$	1293 "	1891 "
	b	$\frac{29}{32} \times \frac{13}{16}$	1104 "	1500 "
4	a	$\frac{13}{16} \times \frac{27}{32}$	1550 "	2449 "
	b	$\frac{13}{16} \times \frac{27}{32}$	1457 "	2302 "
5	a	$\frac{27}{32} \times \frac{25}{32}$	637 " *	944 " *
	b	$\frac{27}{32} \times \frac{25}{32}$	1171 "	1850 "

* Indicates a defective brickette.

SHRINKAGE TEST.

No.		Area Green	Area Burned	Per Cent. Loss
1	a	1. Sq. In.	.736 sq.in	26.40
	b	1. "	.683 "	31.64
2	a	1. "	.736 "	26.40
	b	1. "	.711 "	28.90
3	a	1. "	.683 "	31.64
	b	1. "	.736 "	26.37
4	a	1. "	.632 "	36.72
	b	1. "	.632 "	36.72
5	a	1. "	.675 "	32.50
	b	1. "	.632 "	36.72

TESTS OF SERIES. (Continued)

B-II.

ELECTRICAL RESISTANCE TEST.

No.		Thickness	Puncturing Voltage	Remarks:
1	a	$\frac{3}{16}$ inch	16,650	
	b	$\frac{3}{16}$ "	18,450	
2	a	$\frac{3}{16}$ "	15,975	
	b	$\frac{3}{16}$ "	18,900	
3	a	$\frac{3}{16}$ "	15,300	
	b	$\frac{3}{16}$ "	16,650	
4	a	$\frac{3}{16}$ "	17,100	
	b	— "	——	Punctured at 34,500 Volts, ⅝ in. thick.
5	a	$\frac{3}{16}$ "	19,125	
	b	— "	——	Punctured at 47,250 Volts, ⅝ in. thick.

ABRASION TEST
With 200 Pounds Flint Pebbles in Wet Ball-Mill

No.		Before Test	After Test	Per Cent. Loss
1	a	43.7 grs.	40.0 grs.	8.57
	b	45.5 "	44.5 "	2.20
2	a	45.0 "	44.5 "	1.10
	b	45.7 "	45.5 "	0.55
3	a	42.0 "	41.0 "	2.38
	b	41.5 "	41.5 "	——
4	a	40.5 "	36.5 "	10.00
	b	44.5 "	43.5 "	2.24
5	a	46 0 "	41.0 "	10.87
	b	49.5 "	43.5 "	12.12

a= 2000 revolution test.
b= 1000 " "

ABSORPTION TEST.

No.		Before Soaking	After Soaking	Per Cent. Absorpt'n
1	a	42.0 grs.	47.2 grs.	12.5
	b	40.5 "	45.5 "	11.1
2	a	39.0 "	39.0 "	——
	b	45.7 "	45.7 "	——
3	a	52.2 "	52.2 "	——
	b	41.5 "	47.5 "	——
4	a	55.5 "	55.5 "	——
	b	44.5 "	44.5 "	——
5	a	50.0 "	50.0 "	——
	b	49.5 "	49.5 "	——

TENSILE TEST.

No.		Area of Cross Section	Load Necessary to Break	Tensile Strength of 1 Sq. Inch
1	a	$\frac{15}{16} \times \frac{14}{16}$	623 lbs.	759 lbs.
	b	$\frac{23}{32} \times \frac{15}{16}$	690 "	812 "
2	a	$\frac{27}{32} \times \frac{14}{16}$	1352 "	1971 "
	b	$\frac{27}{32} \times \frac{14}{16}$	1345 "	1962 "
3	a	$\frac{14}{16} \times \frac{13}{16}$	1425 "	2004 "
	b	$\frac{25}{32} \times \frac{14}{16}$	1980 "	2692 "
4	a	$\frac{14}{16} \times \frac{13}{16}$	1565 "	2201 "
	b	$\frac{14}{16} \times \frac{13}{16}$	1421 "	1925 "
5	a	$\frac{14}{16} \times \frac{13}{16}$	1160 "	1631 "
	b	$\frac{14}{16} \times \frac{13}{16}$	1173 "	1590 "

SHRINKAGE TEST.

No.		Area Green	Area Burned	Per Cent. Loss
1	a	1. Sq. In.	.820 sq.in	18.0
	b	1. "	.850 "	15.0
2	a	1. "	.685 "	31.4
	b	1. "	.685 "	31.4
3	a	1. "	.711 "	28.9
	b	1. "	.738 "	26.4
4	a	1. "	.711 "	28.9
	b	1. "	.738 "	26.4
5	a	1. "	.711 "	28.9
	b	1. "	.738 "	26.4

TESTS OF SERIES. (Continued.)

B-III

ELECTRICAL RESISTANCE TEST.

No.		Thickness	Puncturing Voltage	Remarks:
1	a	———	———	Burst in firing.
	b	———	———	
2	a	3/16 inch.	31.050	
	b	1/8 "	25,500	
3	a	3/16 "	Not punct'd	Arced around at 49,500 Volts.
	b	3/16 "	" "	" " at 49,500 "
4	a	3/16 "	" "	" " at 49,500 "
	b	— "	———	
5	a	3/16 "	Not punct'd	Arced around at 49,500 Volts.
	b	3/16 "	" "	" " at 49,500 "

ABRASION TEST
With 200 Pounds Flint Pebbles in Wet Ball-Mill

No.		Before Test	After Test	Per Cent. Loss
1	a	39.5 grs.	38.5 grs.	2.53
	b	45.5 "	45.0 "	1.10
2	a	44.0 "	43.0 "	2.27
	b	45.0 "	43.0 "	4.44
3	a	47.0 "	41.5 "	11.70
	b	57.5 "	57.0 "	0.87
4	a	49.5 "	48.0 "	3.03
	b	56.5 "	55.7 "	1.32
5	a	58.0 "	56.0 "	3.45
	b	51.5 "	51.0 "	0.97

a=2000 revolution test.
b=1000 " "

ABSORPTION TEST

No.		Before Soaking	After Soaking	Per Cent. Absorpt'n
1	a	42.0 grs.	42.0 grs.	——
	b	45.5 "	45.5 "	——
2	a	50.0 "	50.0 "	——
	b	45.0 "	45.0 "	——
3	a	50.7 "	50.7 "	——
	b	57.5 "	57.5 "	——
4	a	58.7 "	58.7 "	——
	b	56.5 "	56.5 "	——
5	a	56.0 "	56.0 "	——
	b	51.5 "	51.5 "	——

TENSILE TEST.

No.		Area of Cross Section	Load Neces'ary to break	Tensile Strength of 1 Sq. Inch
1	a	1/16 x 27/32	825 lbs.	1118 lbs.*
	b	1/16 x 27/32	2070 "	2805 "
2	a	1/16 x 13/16	1220 "	1697 "
	b	1/16 x 13/16	1511 "	2101 "
3	a	1/16 x 13/16	1465 "	2233 "
	b	1/16 x 13/16	1474 "	2247 "
4	a	1/16 x 25/32	1755 "	2569 "
	b	1/16 x 13/16	1715 "	2385 "
5	a	27/32 x 13/16	1504 "	2194 "
	b	13/16 x 13/16	1298 "	1805 "

*Indicates a defective brickette.

SHRINKAGE TEST.

No.		Area Green	Area Burned	Per Cent. Loss
1	a	1.0 Sq.In	.738sq.in	26.2
	b	1.0 "	.738 "	26.2
2	a	1.0 "	.719 "	28.1
	b	1.0 "	.719 "	28.1
3	a	1.0 "	.656 "	34.4
	b	1.0 "	.656 "	34.4
4	a	1.0 "	.683 "	31.6
	b	1.0 "	.719 "	28.1
5	a	1.0 "	.685 "	31.4
	b	1.0 "	.719 "	28.1

TESTS OF SERIES. (Continued.)

C—I

ELECTRICAL RESISTANCE TEST.

No.		Thickness	Puncturing Voltage	Remarks:
1	a	1/8 inch.	34,000	Arced around at 47,500 volts.
	b	1/4 "		
2	a	1/8 "	25,750	
	b	1/4 "	47,000	
3	a	1/8 "	17,250	
	b	1/4 "	18,550	
4	a	1/8 "	22,000	
	b	1/4 "	21,000	Defective.
5	a	1/8 "		
	b	1/4 "	14,900	

ABRASION TEST
With 200 Pounds Flint Pebbles in Wet Ball-Mill

No.		Before Test	After Test	Per Cent. Loss
1	a	36.0 grs.	36.0 grs.	.00
	b	39.5 "	39.5 "	.00
2	a	36.5 "	36.5 "	.00
	b	40.0 "	40.0 "	.00
3	a	41.5 "	41.5 "	.00
	b	41.5 "	41.5 "	.00
4	a	38.5 "	38.5 "	.00
	b	37.7 "	37.7 "	.00
5	a	33.7 "	33.7 "	.00
	b	33.7 "	33.7 "	.00

=2000 revolution test.
b=1000 " "

ABSORPTION TEST.

No.		Before Soaking	After Soaking	Per Cent. Absorpt'n
1	a	36.0 grs.	36.0 grs.	.00
	b	39.5 "	39.5 "	.00
2	a	36.5 "	30.5 "	.00
	b	40.0 "	40.0 "	.00
3	a	41.5 "	41.5 "	.00
	b	41.5 "	41.5 "	.00
4	a	38.5 "	38.5 "	.00
	b	37.7 "	37.7 "	.00
5	a	33.7 "	33.7 "	.00
	b	33.7 "	33.7 "	.00

TENSILE TEST

No.		Area of Cross Section	Load Neces'ary to Break	Tensile Strength of 1 Sq. Inch
1	a	27/32 x 27/32	2094 lbs.	2942 lbs.
	b	27/32 x 27/32	1494 "	2099 "
2	a	13/16 x 27/32	1361 "	1985 "
	b	13/16 x 27/32	2105 "	3071 "
3	a	25/32 x 25/32	1585 "	2404 "
	b	25/32 x 25/32	1330 "	2018 " *
4	a	13/16 x 13/16	1686 "	2555 "
	b	13/16 x 13/16	1543 "	2308 "
5	a	25/32 x 13/16	1373 "	2163 "
	b	13/16 x 13/16	1729 "	2619 "

*Indicates a defective brickette.

SHRINKAGE TEST

No.		Area Green	Area Burned	Per Cent. Loss
1	a	1. Sq. In.	.7118sqin	28.82
	b	1. "	.7118 "	28.82
2	a	1. "	.6855 "	31.45
	b	1. "	.6855 "	31.45
3	a	1. "	.6592 "	34.08
	b	1. "	.6592 "	34.08
4	a	1. "	.6601 "	33.99
	b	1. "	.6601 "	33.99
5	a	1. "	.6348 "	36.52
	b	1. "	.6601 "	33.99

TESTS OF SERIES. (Continued)
C-II.

ELECTRICAL RESISTANCE TEST.

No.		Thickness	Puncturing Voltage	Remarks:
1	a	1/8 inch	———	
	b	1/4 "	13,500	
2	a	1/8 "	14,500	
	b	1/4 "	15,200	
3	a	1/8 "	14,000	
	b	1/4 "	14,300	
4	a	1/8 "	14,250	
	b	1/4 "	23,750	
5	a	1/8 "	18,750	
	b	1/4 "	36,000	

ABRASION TEST
With 200 Pounds Flint Pebbles in Wet Ball-Mill

No.		Before Test	After Test	Per Cent. Loss
1	a	30.0 grs.	30.2 grs.	8.33
	b	33.0 "	29.0 "	12.12
2	a	30.0 "	29.7 "	0.83
	b	30.7 "	30.2 "	1.63
3	a	33.0 "	33.0 "	0.00
	b	28.5 "	28.5 "	0.00
4	a	30.5 "	30.5 "	0.00
	b	30.5 "	30.5 "	0.00
5	a	32.0 "	31.2 "	2.34
	b	30.0 "	29.0 "	3.33

a = 2000 revolution test.
b = 1000 " "

ABSORPTION TEST.

No.		Before Soaking	After Soaking	Per Cent. Absorpt'n
1	a	29.0 grs.	33.0 grs.	13.8
	b	29.0 "	33.0 "	13.8
2	a	29.5 "	30.0 "	1.7
	b	30 0 "	30.7 "	2.5
3	a	33.0 "	33.0 "	——
	b	28.5 "	28.5 "	——
4	a	30.5 "	30.5 "	——
	b	30.5 "	30.5 "	——
5	a	32.0 "	32.0 "	——
	b	30.0 "	30.0 "	——

TENSILE TEST.

No.		Area of Cross Section	Load Necess'ary to Break	Tensile Strength of 1 Sq. Inch
1	a	7/8 x 7/8	843 lbs.	1101 lbs.
	b	11/16 x 11/16	876 "	1145 "
2	a	23/32 x 13/16	1018 "	1604 "
	b	23/32 x 13/16	1084 "	1706 "
3	a	13/16 x 13/16	410 "*	—— "*
	b	13/16 x 13/16	1157 "	1753 "
4	a	23/32 x 13/16	1140 "*	1796 "*
	b	23/32 x 13/16	1643 "	2585 "
5	a	23/32 x 13/16	705 "*	—— "*
	b	23/32 x 13/16	1492 "	2350 "

* Indicates a defective brickette.

SHRINKAGE TEST.

No.		Area Green	Area Burned	Per Cent. Loss
1	a	1. Sq. In.	.765 sq.in	23.44
	b	1. "	.764 "	23.56
2	a	1. "	.634 "	36.52
	b	1. "	.634 "	36.52
3	a	1. "	.660 "	33.99
	b	1. "	.660 "	33.99
4	a	1. "	.634 "	36.52
	b	1. "	.634 "	36.52
5	a	1. "	.634 "	36.52
	b	1. "	.634 "	36.52

GENERAL CONCLUSIONS ON THE WORK AS COMPLETED.

The object of this investigation was to prove, if possible, the proportions of the essential constituents which go to make up the best insulating porcelains. By the test, we have learned that porcelains of equally good appearance may possess greatly varying values when subjected to various tests. The essential properties of an electrical insulator are: That it possess, first of all, a high efficiency as a non-conductor. Second, that it stand, without snapping, any reasonable strain put upon it by the exigencies of wiring. Third, that it withstand the blows of missiles, even bullets, without breaking. The electrical, the tensile and the abrasion tests, respectively, were made to show these properties in the porcelains tested.

From a study of the electrical resistance tests we have found that every specimen standing a superior test has a high silica and moderately low alumina content. Unfortunately in each series showing the variation in proportions of CaO and K_2O, the content of Al_2O_3 was so high that no exceptionally good porcelains were obtained, even with high K_2O content. However, a study of tables A-II, B-II and C-II will show the reader that a marked improvement is apparent upon increasing the K_2O and decreasing the CaO, except at the extreme of the series where we see the K_2O used alone, and here the specimens show weakness due to brittleness. I infer from this study that had the K_2O been somewhat increased, and the CaO proportionately decreased in series I and III, these series would have proved much better.

From a study of the abrasion tests, it is very apparent that with K_2O alone as a flux, and even with a moderately high clay substance, we obtain a brittle and weak porcelain. When as low as 20 per cent of K_2O is replaced by CaO, we find the product changed to a tough but well vitrified body, although I believe that the translucency is reduced by any additions of CaO. To just what extent this is the case is not a matter of importance in this work. In the series showing the effect of Al_2O_3 content, I notice

no great difference in the ability of the specimens to resist abrasion. Much the same statement would apply to the series showing the results of varying of SiO_2. However, I note that the medium high SiO_2 specimens seem to chip worse than the extremely high SiO_2 specimens, while the low SiO_2 specimens show a lack of ability to resist abrasion.

A study of the test tables will show the reader that of the forty different porcelains tested, we find six to be more or less porous. It appears that the statements made in discussing the tensile tests hold here also, and that the same limits may be accepted. I also note that CaO when substituted for an equal equivalent of K_2O, does not at either cone 6, cone 9, or cone 12, produce an equal amount of vitrification.

My conclusions from a study of the tensile tests are that a moderately high SiO_2 and Al_2O_3 content will bring a good strong porcelain, but that not over 1. Al_2O_3 and 6.2 SiO_2 should be used. None of the specimens below .8 Al_2O_3 and 4.2 SiO_2 were safe, owing to their very narrow vitrification limits. The RO that proved most satisfactory in the tensile tests was between

$$\left\{ \begin{array}{l} 40 \text{ per cent. } K_2O \\ 60 \text{ per cent. } CaO \end{array} \text{ and } \begin{array}{l} 80 \text{ per cent. } K_2O \\ 20 \text{ per cent. } CaO \end{array} \right\}$$

The shrinkage tests tend to show, I think, that there are three factors that may cause a variation: (1) The amount of clay substance introduced. (2) The variation in the kind of flux used. (3) The amount of silica present. Hence the shrinkage may be varied to suit almost any case.

In closing this paper, I think I may say that in my opinion an ideal porcelain for electrical purposes, may mature at any point between cone 6 or cone 12. It may contain from 50 per cent. to 80 per cent of its RO in the form of K_2O and the remainder as CaO. It may contain from .8 Al_2O_3 to 1.0 Al_2O_3 and from 4.2 SiO to 6.2 SiO_2. Expressing the same fact in formula form, we would have

$$\left.\begin{array}{l} 0.5 \text{ to } 0.8\ K_2O \\ 0.5 \text{ to } 0.2\ CaO \end{array}\right\}\ 0.8 \text{ to } 1.0\ Al_2O_3 \left\{ 4.2 \text{ to } 6.2\ SiO_2 \right.$$

as the practical range of composition.

Closer limits than these I do not feel able to lay down, as the choice would be largely a matter of fancy for more translucency or more stoniness to be decided by the party interested. However, I think I may safely say that any porcelain within the limits named above, would, all other things being equal, be as good as, if not superior to the average electrical insulator porcelain now on the market.

DISCUSSION.

The Chair: Mr. Watts is certainly to be congratulated upon the able manner in which he has handled this subject. It is the first composition which has ever appeared in this or any other country on porcelain in which the matter of composition and electrical resistance are brought into comparison. I know you are all thinking now how hard it is going to be to discuss such an enormous mass of facts as Mr. Watts has patiently accumulated and presented here today. It certainly deserves some discussion, and an expression from the society of the recognition of its merits. I think Professor Orton would probably be glad to start the discussion. He has probably given some thought to the subject before he came here.

Professor Orton: I cannot say anything about the paper except to express my admiration for the patience and fidelity with which Mr. Watts has carried out his task. It has involved three months' patient labor, and there were a good many occasions upon which he was obliged to sit up all night, in order to carry through a test; and I think I may fairly say that there are not so very many people in this society or outside of it, who would be willing, for the sake of scientific advancement, to devote three months of their time, without recompense, to an investigation of this sort. In my opinion, it marks an era in the life of this society, when a young man of training, opportunity and experience, is willing to present a subject upon which so little is known and upon which so high a commercial valuation is placed.

The transactions of the electrical societies of the world contain records of a great variety of tests on electrical resistance, and the subject has been very carefully studied by

them to find a material to help out in the handling of high tension currents. But so far as I am aware (Mr. Watts will correct me if the statement is not true), no one has before attempted to show the relation between the composition of porcelain, and its ability to resist high tension currents; and I consider it a great honor for the American Ceramic Society to be the medium of presentation to the world of the first authentic information of this character. As far as discussing the paper on its merits is concerned, I am entirely unable to do so.

It occurs to me to ask Mr. Watts to describe to the society the way an electric current acts on arcing around an obstruction or insulation. Can you show us just how that occurs?

Mr. Watts: (Makes illustration on blackboard). Those of you who have had experience with any kind of electrical work know that when a current reaches a certain tension, the tendency is to complete the circuit, no matter what the distance. Electrical engineers have determined the actual voltage necessary to jump a certain distance of air; and as I stated in my paper, about five inches of air space will be arced through, or crossed by the current at a voltage of 55,000; and in order to puncture the insulator, you must have an insulating material extending beyond that distance or make the insulation less perfect. That is the reason so many of my tiles could not be punctured. I hoped that I had made them so thin that the resistance of the porcelain would not be equal to the pressure of fifty thousand volts, but I was mistaken.

The modern high-voltage electrical insulator is made with two or three bonnets or petticoats, surrounding a central hole in which the plug is attached, by which it is fastened to the pole. The electricity passing on the wire, in its effort to get to the pole, if it were not for these aprons, would have sufficient power to arc through the air to the pole, at say thirty thousand volts. But by the use of these large aprons, it must traverse a much longer distance. That is the feature of the bell or petticoat insulator. The arcing consists of the electricity being transported across an

open space. In this case, it will follow the surface of a non-conductor rather than arc through the air. It almost invariably does this, and that is the reason why the bell insulator has such greatly extended surfaces. Does that answer your question, Professor?

Professor Orton: Yes, I think so. I would like to ask what is the relation between these bodies you have been exploiting here, and the general types of bodies in use in the electrical porcelain business. Have electrical porcelain manufacturers developed anything like a type for electrical porcelain, so far, or is each working for himself and keeping quiet?

Mr. Watts: Any man having a porcelain insulator standing fifteen or sixteen thousand volts feels that he has a gold mine, and will not let any information concerning it get out. Manufacturers having a material standing over that voltage have taken the greatest pains to prevent the composition of the body being known. I know of several instances where they have gone to ridiculous extremes to keep the matter secret. The bodies of high resisting porcelains in use in the United States, so far as I am familiar with them, use potash almost exclusively as the flux. Some few may contain a small quantity of lime. The tests I have made strengthen my belief, that a potash-lime porcelain is better able to endure strains than pure potash porcelain. You must understand that insulators are required to carry an immense strain, because high tension currents are carried on very large heavy copper wires, and the blowing of the wind on this wire frequently breaks the head of a large insulator short off.

Professor Orton: Do you know whether a red burning clay of fine vitrifying qualities has ever been used for insulation?

Mr. Watts: I know of one instance where a buff-red clay has been used successfully. But as far as I know, it did not stand a test of more than about 80,000 volts, while the porcelain which I made for this same party stood a test of 120,000. I do not believe a red burning body would stand as severe a test as the white or true porcelain.

Professor Orton: Does the presence of iron in the porcelain form the weak spot? Would the electric current break through a ferrous or ferric silicate easier than through a potash silicate?

Mr. Watts: I cannot say. But I know that any tendency to small cavities in the porcelain weakens it remarkably in resisting strains. It must be thoroughly vitrified so that moisture cannot penetrate it. If the moisture penetrates it, it decreases its resistance very fast. Dr. Zimmer cited a case last year of a kaolin vitrifying at a very low temperature. I happened to have some of the same sort of clay myself, and I put a piece of the raw clay into the kiln, and fired it to cone 02 and it came out magnificently vitrified. I never saw a piece of porcelain break with a more conchoidal fracture, yet it was somewhat ferruginous.

Professor Orton: What I want to know is whether a body of that kind would have a high insulating character?

Mr. Watts: That I do not know. I do not believe from what I have seen that the presence of iron had any deteriorating effect on the body used. I know of a number of clays being used in electrical insulating bodies, which have quite noticeable iron specks in them; and while these are not segregated and are evenly distributed, at the same time there is no indication of a deterioration of the product, owing to their presence, unless there is a big bubble or cavity of some sort formed by them.

Mr. E. C. Stover: Year before last, when we were at the power plant at Niagara Falls, I got hold of the head electrician there and asked him some questions along this line, as regards the different make-up of porcelain insulators and their relative resistance to electrical current, and his reply was that it was a case of mechanical strength rather than a case of chemical constitution. He claimed that perfect glass would make a thoroughly good insulator, but it was not used, because it was not so strong as a good solid porcelain. And I think some of the tests given there show that point quite clearly, as regards mechanical strength.

THE USE OF LEAD ORES IN CERAMICS.

BY

H. A. WHEELER, E. M., ST. LOUIS, MO.

In the use of lead in raw glazes, fritts, or glasses, for obtaining low fusing points, or soft enamels, or a highly vitreous lustre, the usual compounds employed are the oxides, litharge (PbO) and minium or red lead (Pb_3O_4), or the carbo-hydrate, white lead ($2\ PbCO_3$, $Pb\ (HO)_2$), which are all artificial products. When obtained from reputable dealers they are usually reasonably pure, the impurities* seldom exceeding three per cent. and often less than one per cent.

They generally come fine enough for immediate use, without further grinding, and they can be used direct for either raw or fritted glazes. But they are liable to cause lead poisoning if there is a lack of cleanliness in their use, and they are expensive, costing from five to ten cents per pound, according to the quality, quantity and market.

The natural ores of lead frequently occur sufficiently pure that they can be successfully substituted for these manufactured compounds in most of the requirements of ceramics, while two them, the sulphate or anglesite and the sulphide or galena, are free from poisonous effects, which alone should cause them to be used wherever it is possible, in preference to the manufactured compounds. These ores, or natural compounds of lead, are:

Cerussite, or "lead dry-bone," or the carbonate of lead.

Anglesite, or "hard carbonates," or the sulphate of lead.

Galenite, or galena, or "mineral," or the sulphide of lead.

There are other lead minerals that can be found in museum collections, but they occur in too small quantities and are too scarce to interest the ceramist.

* The dry white lead, not ground in oil, is of course alluded to. It is far less apt to be adulterated with baryta, as often occurs in the oil-ground product.

The carbonate, or cerussite ($PbCO_3$), contains 83.5 per cent. PbO or oxide of lead (or the active fluxing constituent in all natural or artificial lead compounds) when pure, and it can be used raw or in fritts or fused glazes direct without further preparation than grinding. It is a soft white, grayish or brownish colored mineral, that has a non-metallic lustre, and is very friable or easily ground. It is found in very large quantities in many mines in the Far West, and, like the sulphate or anglesite, is found in the upper portions of the mine, but never in depth or below the water-level, where the lead always changes to the sulphide or galenite. Notable occurrences are found in Leadville and in Chaffee county, Colorado, at Bingham and Tintic, Utah, and at Eureka, Nevada. In the Mississippi Valley lead districts of Missouri, Iowa and Illinois, this carbonate ore is only found in small amounts, though occasionally in ample quantities to meet the requirements of the ceramist.

As found in nature, carbonate ore is usually so mixed with the brown oxide of iron (limonite) and sometimes clay and other silicates, as to be valueless in ceramics; but pockets of many tons occasionally occur of a white to light gray color, that carry less than 5 per cent. of impurities, and hence, could be substituted for the more expensive manufactured compounds in many cases. Like the artificial carbonate and oxides of lead, cerussite is poisonous if taken internally, or absorbed through the skin, if lead poisoning is possible by such means, which will be taken up later.

The sulphate or anglesite ($PbSO_4$) contains when pure 73.6 per cent of oxide of lead, and it can be used direct in raw or fused glazes after grinding. It is a soft white to dirty gray colored mineral that has a non-metallic or earthy lustre, is usually very compact and heavy, but grinds readily. It is found quite abundantly in the far West, often in the same mine with the carbonate; Aspen and Eagle River, Colorado, and Frisco, Utah, have produced large quantities. The Mississippi Valley lead districts only produce it in trifling amounts, usually as a dirty gray exterior or shell on the galena or sulphide.

The sulphate is less apt to be contaminated with iron and other impurities than the carbonate, and by careful hand picking one would frequently be able to sort out several tons in many of the mines, that would have less than 5 per cent. of impurities. The dirty, dark color that it frequently has would suggest gross impurities, but the latter will often be found to aggregate less than 3 per cent. in very unattractive looking material. In the writer's opinion, the sulphate of lead is not poisonous, which makes this salt a very desirable form to use, whether the natural or artificial sulphate is employed.

Galenite or galena or the sulphide of lead (PbS) contains 86.6 per cent. Pb of metallic lead when pure, or 95.3 per cent. PbO or oxide of lead when the sulphur (S) has been driven off in an oxidizing atmosphere such as is almost invariably used in ceramics. Before the lead oxide in galenite or the sulphide of lead is available, it is necessary to roast and drive off the sulphur with which it is combined, which is accomplished by heating it to a red heat with an excess of air. As this is the condition of about all kilns or muffles when being heated, the galena or sulphide can be used direct in raw glazes, as the sulphur will roast and be driven off by the time the glaze has melted. But in fritting or fusing in crucibles, where the air is more or less completely shut off, galena or the sulphide can not be used, as most of it will melt and settle to the bottom as a matte in a raw or uncombined condition, since the silicic, boracic or phosphoric acids that all glazes depend on to make them fusible, are not able to decompose the sulphide of lead, and, therefore, the sulphur is not expelled. Where it is possible to use such oxidants as potash or soda nitre in the charge, the sulphur is expelled or else converted into the harmless SO_2 condition, and this valuable lead salt can then be employed for fused glazes, as all the lead is then rendered available. *

* If nitre is used to drive off the sulphur in galena, care must be exercised to not heat the charge too rapidly, least the combustion of the sulphur causes such a rapid evolution of SO_2 or SO_3 gases as to result in the charge boiling over.

Galena or galenite is a bluish-gray, metallic, very heavy mineral that is very brittle and quite soft. It occurs very abundantly in most of the mining districts of the far West, where it is usually far more valuable for its silver than its lead, as it may contain from 20 to 500 ounces of silver per ton, which is true of all western lead ores, whether sulphide, sulphate or carbonate. In the Mississippi Valley it is free from silver, while it usually occurs purer than in the far West, and it is very abundant, from the ceramic standpoint, in southeastern, central and southwestern Missouri, northwestern Illinois, southeastern Wisconsin, and northeastern Iowa. Excepting in southeastern Missouri, the galena is usually associated more or less intimately with zinc and iron minerals, that would gravely interfere with its ceramic value.

In the southeastern Missouri district, of which St. Louis is the outlet, the lead occurs in two radically different forms or types of deposits, known as the "disseminated" and "surface" mineral. The "disseminated" type of galena deposits occurs notably in St. Francois county, where the lead is found as a very low grade ore finely scattered or disseminated in a magnesian limestone. This is crushed and concentrated by water in mills to a product that will assay from 60 to 75 per cent. lead, the practice varying at the different mills, so that there are still 12 to 20 per cent. of impurities, which is mainly fine magnesian limestone of quite uniform character. There is a product that comes from the setting boxes of the slime machines in the concentration mills that is in a very fine condition, and which in some of the mills will assay 75 to 78 per cent. of lead, and which would be the most attractive material for the ceramist from this district.

The "surface" type of galena is mainly found in Washington and Jefferson counties, and Palmer and Potosi are the principal producing points. This surface or "block-mineral" consists of large cubes and chunks of galena that are found in the ferruginous or red surface clays, and which after a limited amount of picking with a cleaning hammer, or washing with a hose or water-bucket, will assay 80 to 85

per cent. in lead, or contain only 3 to 8 per cent. of impurities. By specifying a *specially selected* or extra clean mineral free from clay, carload lots of this coarse, lump galena can be secured that will assay 84 to 85.5 per cent. lead, or contain only 1½ to 3 per cent. of impurities, or pure enough for almost every use of the ceramist. The usual prices of the market run of this "surface" galena vary from $40.00 to $50.00 per ton, while by paying one-half cent. a pound extra, the very pure extra clean mineral that assays about 85 per cent. lead can be secured at, say three cents per pound, which is the richest lead compound on the market, excepting the two oxides minium and red lead, and by far the cheapest.

The galena or sulphide of lead is not poisonous from absorption through the skin and *probably* is not, when taken internally. The writer has strong doubts whether the lead poisoning, so frequently found among painters, lead-glaze dippers, lead smelters, etc., is ever due to absorption through the skin. For while the hands of this class of workmen are frequently more or less covered with oxide or carbonate compounds of lead, which would permit of its ready absorption through the pores, an intimate acquaintance with lead workers, leads the writer to believe that the lead is introduced into the human system by the food and tobacco that is handled by these same lead-befouled hands. It is a most painful fact that familiarity causes such men to become very careless and indifferent to the evils of lead poisoning, so that a man will sit down and eat his lunch without taking the trouble to first wash his hands, although they may be grimy with lead. The constant handling of tobacco, also, without any effort to protect the surface from contamination is a prolific source of trouble. Even where the wash basin is freely used, the dirt under the finger nails is seldom carefully attended to, and while this source of poisoning the food does not operate so rapidly as the unwashed hand, it is always loaded, and slowly but surely gets in its disastrous cumulative work. It is especially this source of contamination that causes the writer to doubt the poisonous character of galenite when taken internally, since his experience with many hundreds of workers in the lead mines and

concentration mills has failed to show any poisonous results. Yet, as soon as this sulphide is converted more or less freely into carbonate and oxide in the roasting and smelting furnaces, lead poisoning becomes extremely common among the same class of men, although the lead miners are even more careless than the lead smelters about washing their hands before eating. It would therefore seem that the lead mine workers probably take considerable more lead into their system in the course of a year than the lead smelters; but as it is in the form of galenite or the sulphide, which is insoluble, and as the gastric juices seem to have no effect on it, it seems to pass through the system and is promptly thrown off without producing any poisonous effects, or accumulating in the manner which makes the oxide and carbonate forms of lead so dangerous.

The writer further believes that the sulphate of lead, whether natural or artificial, is as free from poisonous action as the sulphide, as it is a salt of even greater insolubility; but his personal experience is not so extensive with this salt of lead. The writer is well aware of many lead mines in the far West that are most disastrous on the health of the miners, from the different forms of lead poisoning that result from working in them. In one case in Utah, from four to six weeks was as long as a man could withstand it, before being returned to the hospital. While in some of these cases, the sulphate or anglesite ore occurred, the writer always found it freely associated with the cerussite or carbonate, and, in the very dangerous mines, it was always the carbonate form. As these oxidized forms of lead ores are only found in *dry* mines, the severely poisonous character of such mines is usually due to the lead dust that floats in the mine atmosphere, resulting from the blasting, especially as lead ore is a favorite tamping for the blast holes.

The writer warmly commends the sulphide or sulphate compounds of lead to the ceramist to avoid the evils of lead poisoning, and as the sulphate will be found preferable, if not essential, in all crucible work, the artificial sulphate should be used if a satisfactory quality of the natural cannot be procured.

DISCUSSION.

Mr. Hensel: We are aware that American factories are consuming vast amounts of raw lead glazes, and it seems to me it would be of interest to discuss this question. In Germany, dealers in ceramic materials advertise that they have lead ores of high grade for sale which are reliable. We have nothing of that kind in America, and in buying lead ore for ceramic purposes we have to take the chance of getting a good grade. That is a great drawback. It seems to me that if those owning lead mines and putting the ore on the market would take some pains to learn what we need, and to supply a strictly uniform article, they could easily build up a very satisfactory branch of trade which is now entirely overlooked.

Professor Orton: I do not know whether Professor Wheeler is prepared to prove his statements or not, as to the non-poisonous quality of galena. If it can be proven by a collection of medical testimony, or by any other means such as doctors employ to find out such facts, it would be a valuable thing to know. And further, if we should use the sulphide of lead, as Professor Wheeler suggests, as a glaze ingredient, we would only be going back to the original practice of the English potter. The poisonous character of lead glazed ware in the beginning was due to the fact that the potter supplied no silica or alumina with the lead, which took up from the body itself enough to more or less completely satisfy its needs. Now, you can readily appreciate the fact that where the potter dusted powdered galena on a freshly thrown piece with his hands, he would get it very uneven, thick in some places and thin elsewhere. Where the glaze was thick, it was basic, and where it was thin, it satisfied itself with silica and alumina from the body. So those old English crocks were sometimes safe, and sometimes they were not. In consequence, all lead-glazed crockery came into very ill odor in England. But the ease with which glazes can be made with lead has caused potters to resist all attempts to down it by legislation in England and elsewhere, and it is still used and likely to be used.

The question is, cannot this suggestion of Professor Wheeler find a proper reception among individuals using lead glazes for the cheaper line of products—architectural products, glazed brick, the lower grades of floor or wall tiles, such as are used in foreign countries on a large scale, roofing tiles, &c. Why could not these industries use galena, and obtain a glaze which would be good enough for their purposes? It is not necessary that a crude architectural product have a glaze as fine as dishware, which is to be scrutinized at close range.

Mr. Hensel: Galena gives us as good a glaze as any other form for such wares as roofing tile.

Professor Orton: I never saw it tried, bnt I am glad to hear Mr. Hensel say that. I suppose the only objection would be the sulphur, but if that were properly roasted out, there is no reason why the lead oxide remaining is not as good as if introduced from any other source.

TIN OXIDE IN CERAMIC FLUXES.

BY

STANLEY G. BURT, CINCINNATI, O.

The main object of this paper is to determine whether any stannic oxide is soluble in a glaze and not to discuss tin enamels in general, as might be supposed. It is customary among potters to consider tin oxide as wholly insoluble in a glaze. The glaze in its fused state is thought to be unable to dissolve the tin at all, the particles merely floating in it, or as we say, being held in suspension.

It may possibly not be strictly correct to speak of a glaze forming a solution, since we form solutions of solids or gases by dissolving them in a liquid, so a glaze which when cooled is a solid, could not form a solution. However, right or wrong, it is the constant practice to speak of a substance as soluble or not in a glaze, by which we mean that on adding this substance to our glaze, we either have a clear transparent glaze or else we have the substance undissolved, held in suspension, and an opaque glaze or enamel resulting.

This being understood, it will be plain that our ability to make a tin enamel must rest upon its insolubility in the glaze flux. Now the points to be considered in this paper are, *first*, is it completely insoluble, and *second*, is it insoluble as the dioxide, or does it undergo some chemical change?

In these days we can no longer speak freely of substances as insoluble or infusible. Flint and carbon are now fused, and German chemists make elaborate analyses of the amount of glass dissolved by water. So it seemed quite possible that a certain percent of tin oxide might be soluble in a glaze.

If we turn to the subject of tin in our chemistries, we find that stannic oxide is infusible and insoluble in acids and alkalies, but when fused with sodium hydrate it yields a

stannate, soluble in water. Tin is one of the intermediate elements that act as acids toward strong bases, and as bases to strong acids, so we have sodium stannate, and stannic chloride. Careful research through such works as were at my command, failed to throw any light on the subject of this paper.

It is quite obvious that the first thing to investigate is whether this tin oxide was absolutely inert in ceramic fluxes. That is, whether some chemical change takes place in the oxide when fused in a ceramic flux, even though it be insoluble in its changed form.

A word in explanation of what I mean by a ceramic flux. In these, I include glazes and their low fusing ingredients considered separately, and exclude such fluxes as are not ordinarily used in ceramics. We know, for instance, that the oxide of tin fused with sodium hydroxide yields a soluble salt, but sodium hydrate is not used by potters, and hence is not a ceramic flux.

Before testing the oxide in actual glazes, I made a few tests, trying first boracic acid calcined and uncalcined. As there was no alkali in this, there was no chance of forming a stannate, and so far as my knowledge goes, we have yet to form a tin borate. Still, I was anxious to see whether there could be any action at all, and was rewarded by an interesting result. I took first the flaky boracic acid, which in heating, melts in its water of crystalization, boils and gradually loses this water, requiring a high temperature to do so completely, and finally leaving as a result, a fine clear glass. The formula for this acid is B_2O_3, 3 H_2O. With the water driven off in calcining, we have left the B_2O_3. Now, to 1. Eq. of the flaky acid, I added 0.004 equivalents SnO_2. The fusion takes place as before, but the result is a fine white enamel. The interesting point is that I believe the stannic oxide has undergone a chemical change, for while the regular oxide is so completely insoluble, this enamel that I got was soluble in HCl, from which solution the Sn was precipitated as the orange sulphide with sulphuretted hydrogen. We had B_2O_3, 3 H_2O and added SnO_2. Is it possible that stannic acid (H_2SnO_3) is formed? It is hardly

possible that the acid could exist at a high heat without dissociation. The water of crystallization is, however, a necessary part of the reaction, for on testing, the calcined boracic acid was unable to show any action on the oxide at all.

In repeating my experiments, to test my results, I found some difficulty in reproducing this reaction with the flaky acid, and as my time was too limited to determine the cause of error, I would not give this test here at all, except for the possible light it may throw on the different appearance of the two enamels. That is, on contrasting the enamel formed from the calcined acid with that from the non-calcined, we find that in the former the actual particles can be noticed in suspension, while in the latter they are altogether too fine to be distinguished. It has, in fact, quite the look of a precipitate.

Borax was next tried. Here we introduce sodium into our equation. The borax on fusing acts much the same as the boric acid; that is, it melts, gives off its water of crystallization, the result being finally the well known clear bright glass. On adding the stannic oxide to it, however, the action is entirely different. In the first place, I was unable to produce the voluminous white enamel produced with the flaky acid, but on the other hand I was here able to produce a perfectly clear solution of the tin in the borax. We have in borax Na_2O, 2 (B_2O_3), 10 H_2O. To this we add SnO_2. I would like to have shown the same effect from the water of crystallization as shown with the acid, but there is less actual boiling, if I may so call it, and the swelling of the borax gives less chance for action on the tin in that state. What we do form here seems to be the sodium stannate, $Na_2\,SnO_3 + 2\,(B_2O_3) + 10\,H_2O$, which gives a clear solution. Naturally, if too much tin is taken, the flux will be unable to dissolve it all, and the excess will be merely held in suspension, and an enamel will result. I was able to dissolve 0.0129 Eq. SnO_2 in 1. Eq. of Borax. This is of course far below what the formula calls for, but the action is very slow, and a prolonged fusion would be necessary to determine just how much could be dissolved.

Calcined borax acted equally well, showing that the water of crystallization is not a factor in this reaction. I feel justified in assuming the formation of Na_2SnO_3 here, for if there was any chance of a tin borate being formed, that would occur with the boracic acid.

Here we have a clear case of the complete solution of a certain per cent of the stannic di-oxide in a ceramic flux. That is, in this flux, we have converted it to a stannate which was soluble.

Those of us familiar with color-making know that in certain receipts for the tin-chromium-pink color, borax plays a leading part; that is, its action on the tin enables us to form the color. I have here a sample of borax and tin, fired together without any chromium, in which you see the distinct formation of a lilac color.

We know that by fusing the tin oxide with NaOH, we have a ready means of forming the sodium stannate. While, as before stated, NaOH is not a ceramic flux, Na_2CO_3 is, so it seemed of interest to determine what its action would be. When fused alone, Na_2CO_3 will not give a clear glass on cooling, so it is impossible to see in what form the tin fused in it is present. The mass resulting from this fusion is only partially soluble in water. In this solution I could find no tin, but the total mass was soluble in HCl. In just what form the tin is present here, is hard to say. Its not being soluble in water would indicate that it was not the stannate. What is plain, however, is that we have here another ceramic flux which acts directly on the tin oxide.

As a final preliminary test, I added SiO_2 to the Na_2CO_3 until all effervescence ceased, forming thereby, sodium silicate. Now, to this I added tin oxide, aiming to determine thereby whether this silicate would have any action on the tin. I found that the action there was practically the same as with the carbonate — partial solution in water, complete solution in HCl. The tin easily precipitated as orange sulphide with sulphuretted hydrogen. This is a most important test. All glazes are silicates. Therefore, if in a silicate there was no direct action on the oxide, this

investigation would necessarily be much more complicated. We would very possibly be ruled out of court as having no case.

We would have to show that the tin went into an early combination, and in that way was soluble in the silicate flux. Failing in this, a careful investigation of various silicates would be necessary, a long and difficult problem. Fortunately, this is not necessary, as this test shows that the oxide of tin is not an inert substance in a silicate flux, and further we have shown that in the presence of borax, it may go into a complete solution. My next tests show that even without borax, we may get a complete solution.

I now tested two regular glazes with the stannic oxide. The first, a fritted glaze, having 1. equivalent B_2O_3, the second, a raw lead glaze, having no B_2O_3. To these I added the SnO_2, starting with 0.01 equivalent and running up to 0.25 equivalent. I have samples of these with me, and would be glad to have the members examine them. In the raw lead glaze, the 0.01 equivalent seems to be completely dissolved, while with 0.02 we can easily see a trace remaining. The 0.25 equivalent gives a complete enamel. In the fritted glaze not only is the 0.01 completely dissolved, but as far as I can determine, 0.02 has also gone into solution. It is therefore probable that the average glazes dissolve at least 0.01 equivalent of tin oxide.

The enamels formed by these two glazes are quite different. While the fritted glaze forms a fair enamel the raw glaze has a curdled look, and the small particles of tin are easy distinguishable. One of the points I hoped to prove in this paper, was my belief that certainly some of the tin was first dissolved and then precipitated in a true enamel. In speaking of the enamels from the calcined boracic acid, and that from the un-calcined, I called attention to the fact that while the former showed the particles in suspension, the latter had the true milky character. Much the same effect is noticed in the enamels from these two glazes. However, this is merely theory, for which as yet I have been unable to gather sufficient proof, so I will not dwell upon it further.

It may be asked what the direct practical value of such an investigation is. This question of the solubility of tin oxide was raised in a discussion of crystalline glazes. We know that certain elements combine with silica to form amorphous silicates, which give a clear glass or glaze. Now this combination takes place at a high heat. Hot liquids, as a rule, are able to hold in solution a greater per cent. of salt than cold ones, so that we often have a clear hot solution from which salts crystallize out on cooling. The same seems to be true of a glaze. While at its maximum heat, a glaze may be clear, even if supersaturated with certain elements. On slow cooling these may crystallize out, forming at times very beautiful results. We have been able to produce this effect from a number of elements, as for instance, zinc, iron, copper, chromium, titanium, but others remain to be investigated. If we are ever to attempt any such result from tin, we must of course first show that the silicate flux has a solvent action on the tin oxide, second, a proper supersaturated solution must be formed, and third, this must be tested with the careful cooling required by other crystalline glazes.

I feel that the first step has been accomplished, as it has been shown that a certain per cent. of tin is soluble in a glaze. The formation of a proper super-saturated solution should next be tried. As the sodium stannate is shown soluble in borax, it is possible that a super-saturated solution of this, added to a fritt under proper conditions, might lead to satisfactory results.

DISCUSSION.

Mr. Stover: (Placing on the blackboard the following formula:)

$$\left.\begin{array}{l} .60\ CaO \\ .36\ K_2O \\ .04\ SnO_2 \end{array}\right\} 0.5\ Al_2O_3,\ \ 4.00\ SiO_2 \qquad \text{(At Cone 12)}$$

There is a formula of a tin glaze which matures properly, making a good opaque glaze, at cone 12. The tin oxide in this glaze figures out about 1.5 per cent.

Mr. J. W. Hasburg: A paper involving such questions in chemistry is not very easy to discuss off-hand, but I will state some observations made by me a long time ago on tin glazes.

As stated by Mr. Burt, sodium and tin oxides in proper proportions form sodium stannate, a definite chemical compound soluble in water. Boracic acid and tin oxide in any proportion does not form a chemical compound, as all the boracic acid can be dissolved out, leaving the tin oxide unchanged, which shows that no chemical union has taken place. If, however, an alkali is added to this boracic acid-tin mixture, the tin will be dissolved in proportion to the alkali added.

Tin oxide is the most difficult oxide to dissolve in a silicate, and when more tin oxide is present than the alkali will take up, the mass becomes opaque; sometimes it comes from the fritt fire almost clear, and only becomes opaque after reheating.

On a close examination, it will be found to have a crystalline structure. If other bases having a greater affinity for alkaline silicate, or which dissolve more readily in it, are present, it will not require so much tin oxide to make this silicate opaque.

Titanic oxide acts very much the same as tin oxide, and other bases will also make opaque mixtures if added in excess, but they may require annealing to bring out the opacity and cause crystallization.

The opacity of tin glazes is due entirely, in my opinion, to the tin being in suspension.

The amount of tin oxide in the above formula is small on account of the alkaline silicates being saturated with other bases, which combine with the silicates more readily than tin oxide.

Mr. Hensel: Schumacher states that a lilac color like that which Mr. Burt shows is very often due to a combination of lime, tin and iron. A very small quantity of iron is all that is needed to develop the color.

NOTE ON THE RELATIVE POINT OF DEHYDRATION OF PURE AND CALCAREOUS CLAY.

BY

WILLIAM M. KENNEDY, BEAVER FALLS, PA.

The first experiment on this subject appearing in an American publication, was one performed by Mr. A. E. Barnes. It appeared in the "Clay Worker" for June, 1895. The incompleteness of Mr. Barnes' work was due to his having used a Brown pyrometer of the expansion bar type, the temperature limit of which is about twelve hundred degrees Fahrenheit. The dehydration period is generally construed to include, not only the driving off of the combined water from pure clay, but also the expulsion of CO_2SO_2, etc., from impure clays, such as calcareous or limey clays, ferrugineous clays and the like.

In this experiment I have made a comparison of two of these clays, namely, pure clay and calcareous clay.

I prepared two series of brickettes. The first series was made by grinding in a ball mill 3000 grams of "Florida kaolin," running this slip through a 150 mesh sieve and drying in plaster moulds. The clay was then thoroughly wedged and kneaded and sixty brickettes were made. The brickettes were moulded in the form of elliptical discs with rounded edges, about three-eighth of an inch in thickness and weighing in the neighborhood of thirty grams each. A one-fourth inch hole for inserting wire for removal from the kiln was made in each brickette, and each was carefully stamped with a number for identification. When thoroughly dry, the brickettes were carefully sponged, and polished with a cloth, to prevent loss in weight by chipping of corners and edges in handling.

The other batch of material had the following composition:

Florida Kaolin	70 parts.
Whiting	30 "
	100 parts.

This material was mechanically treated, moulded into brickettes, dried, marked and polished in the same manner as the first batch.

The dried brickettes were heated for one hour in an air oven at 130 degrees C. They were then cooled in a dessicator, and rapidly weighed to prevent absorption of hygroscopic water. The accuracy of weighing was to one milligram. The brickettes were then placed in a muffle in a down draft kiln. A fire-clay tube, three inches in diameter, containing the porcelain tube and thermo-couple of the Le-Chatelier pyrometer was laid lengthwise through the centre of the muffle, and the kaolin brickettes were now piled up on one side of the tube, and the kaolin-lime brickettes on the other side. The distance of the brickettes from the outside of the muffle was about twenty-four inches, and was in the centre of the heated area and well protected from drafts of cold air. The front of the muffle was left open, and opposite it, two large peep holes were left in the wicket, through which it was easy to reach in with a wire and remove the brickettes.

The brickettes being placed in position, the kiln was started. The temperature was carefully noted at every 50 degrees from 100 degrees to 300 degrees, and two of each kind of brickettes were withdrawn at each temperature reading, cooled at a dessicator and weighed. From 300 degrees to 850 degrees brickettes were withdrawn at every 25 degrees. From 850 degrees to 1000 degrees, the completion of the burn, brickettes were removed at every 50 degrees.

The time occupied by the experiment was twenty-eight hours. The temperature was increased as steadily and regularly as possible until 725 degrees was reached, when this heat was maintained as near as possible for a trifle over twelve hours, to observe the effect of heat-soaking on the brickettes. The temperature was then raised rapidly to 1000 degrees.

During the early stages of the burn difficulty was experienced in seeing the brickettes in the kiln, and a bicycle lamp was used to project light into the dark muffle. From 575 degrees up to the completion of the burn, no trouble was

experienced from this cause. In the early stages of the burn, the brickettes were sooty; this disappeared at 475 degrees in the calcareous clay brickettes, and at 500 degrees in the kaolin brickettes. When weighed after removal from the kiln, this soot was removed to as great an extent as possible with a fine "camel hair" brush. At 525 degrees the clay-lime brickettes showed a faint red glow. The kaolin brickettes did not show evidence of becoming "red hot" until 550 degrees was reached.

ANALYSIS OF THE RESULTS.

Taking the kaolin brickettes first, it will be observed (Plate I, black line,) at 150 degrees, the temperature at which the first observation was made, that only .018 per cent. of loss had occurred. From here to 450 degrees the loss was gradual, only amounting to 1.59 per cent. At this point, the brickettes began to lose weight rapidly. This rapid loss continued until a temperature of 600 degrees was reached, when 12.1 per cent. loss had occurred. From 600 degrees to 725 degrees the loss was again very gradual, the temperature was held near 725 degrees for about twelve hours in order to observe the effect of soaking on the expulsion of water. This, as it will be observed, was very slight, only .07 per cent. of loss occurring from this temperature to the end of the burn. Small losses occurred at each observation, aggregating in all only .54 of one per cent. The total loss was 13.84 per cent. and the dehydration period, or the temperature between which the bulk of the combined water, was driven off can with safety be said to lie between 450 and 600 degrees centigrade.

Now taking up the kaolin-lime brickettes (Plate I, broken line) the losses will be observed to be very slight up to 450 degrees. At 475 degrees but very little additional water had been expelled from these brickettes. From here to 600 degrees the rate of loss coincides almost exactly with that of the kaolin brickettes. As 600 degrees represents the close of the dehydration period, the losses from here on are due to the expulsion of CO_2 gas. The dotted line extending from 8.97 percent at 600 degrees to 9.7 percent at 1000

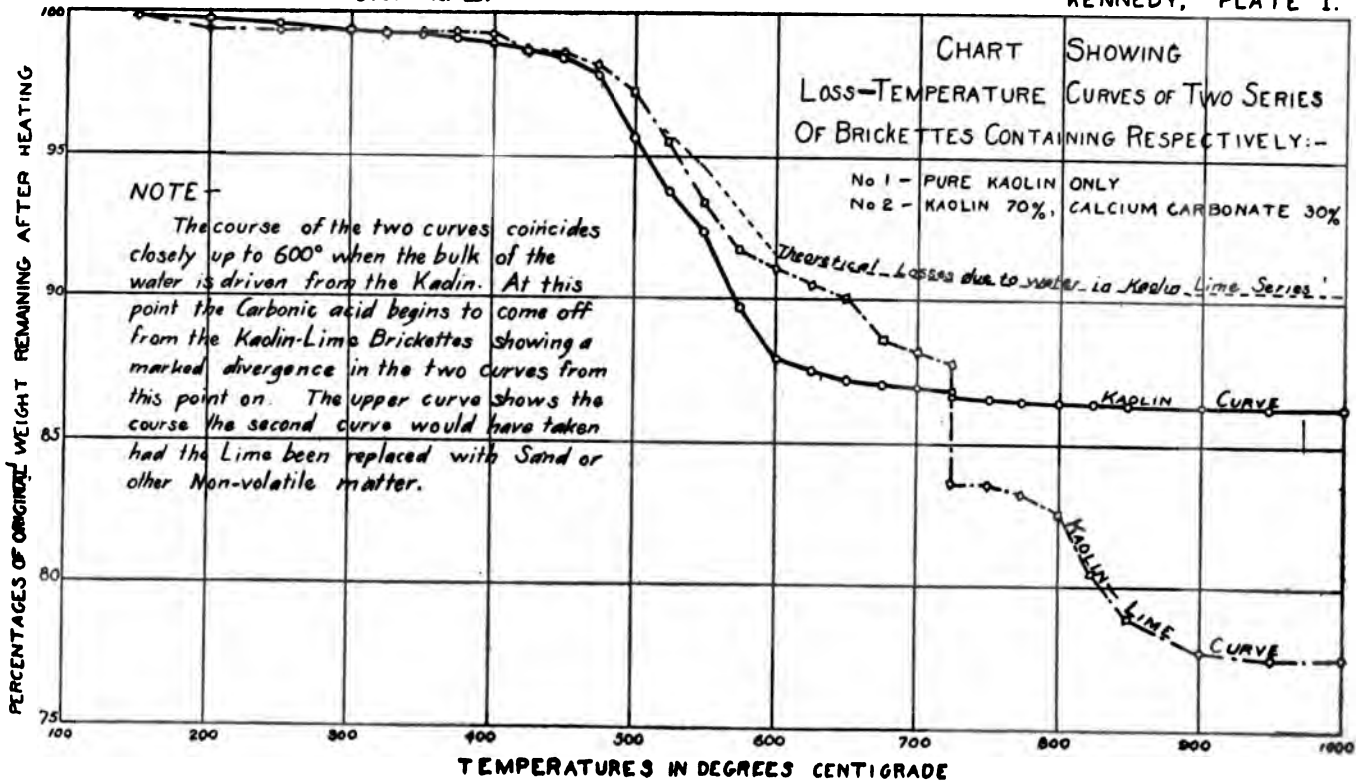


CHART SHOWING
LOSS—TEMPERATURE CURVES OF TWO SERIES
OF BRICKETTES CONTAINING RESPECTIVELY:—
No 1 — PURE KAOLIN ONLY
No 2 — KAOLIN 70%, CALCIUM CARBONATE 30%
NOTE—
The course of the two curves coincides closely up to 600° when the bulk of the water is driven from the Kaolin. At this point the Carbonic acid begins to come off from the Kaolin-Lime Brickettes showing a marked divergence in the two curves from this point on. The upper curve shows the course the second curve would have taken had the Lime been replaced with Sand or other Non-volatile matter.
Theoretical Losses due to water in Kaolin Lime Series
KAOLIN CURVE
KAOLIN LIME CURVE
PERCENTAGES OF ORIGINAL WEIGHT REMAINING AFTER HEATING
100
95
90
85
80
75
100
200
300
400
500
600
700
800
900
1000
TEMPERATURES IN DEGREES CENTIGRADE

degrees represents the theoretical loss of the remainder of water in the kaolin-lime brickettes. From 600 degrees to 725 degrees, the expulsion of CO_2 was almost as rapid as that of the water had been. 12.2 percent loss had occurred up to this point. The soaking period of 12 hours at this temperature shows a decided loss in the kaolin-lime brickettes—4.1 percent out of a total of 22.5 percent; but for the next 125 degrees raise in temperature very little loss occurred. From here to 900 degrees the loss was again rapid, 22.3 percent having occurred at this temperature. From here on to the end of the burn (1000 degrees) only 0.2 percent loss was found. This series of brickettes proves that while the water of dehydration in kaolin is expelled between 450 and 600 degrees, the CO_2 in brickettes containing 30 percent lime will prolong the dehydration period to 900 degrees. It also shows that heat soaking for a comparatively long period is of no advantage, because while it does drive off some of the CO_2, it does not drive off all of it, and the end of the dehydration occurs at the same temperature as if no soaking had been given.

Plate II was drawn for the purpose of showing what took place during the soaking period at 725 degrees.

The solid black line shows the time-loss curve of the kaolin brickettes. It will be observed that the bulk of the water was driven off in less than two hours, or between five and seven hours after lighting the kiln; and it will also be observed that the soaking period had very little effect on the brickettes. From 8 hours and 55 minutes to 28 hours the curve line is almost straight with a total loss of 1.74 percent during the period.

With the kaolin-lime brickettes it is quite different. It will be observed (broken line) that the loss continued at a fairly rapid rate during the whole of the burn, and that during the soaking period, 11 hours and 30 minutes to 24 hours, there was a loss in weight of 4.1 percent.

The following tables show my observations as recorded:

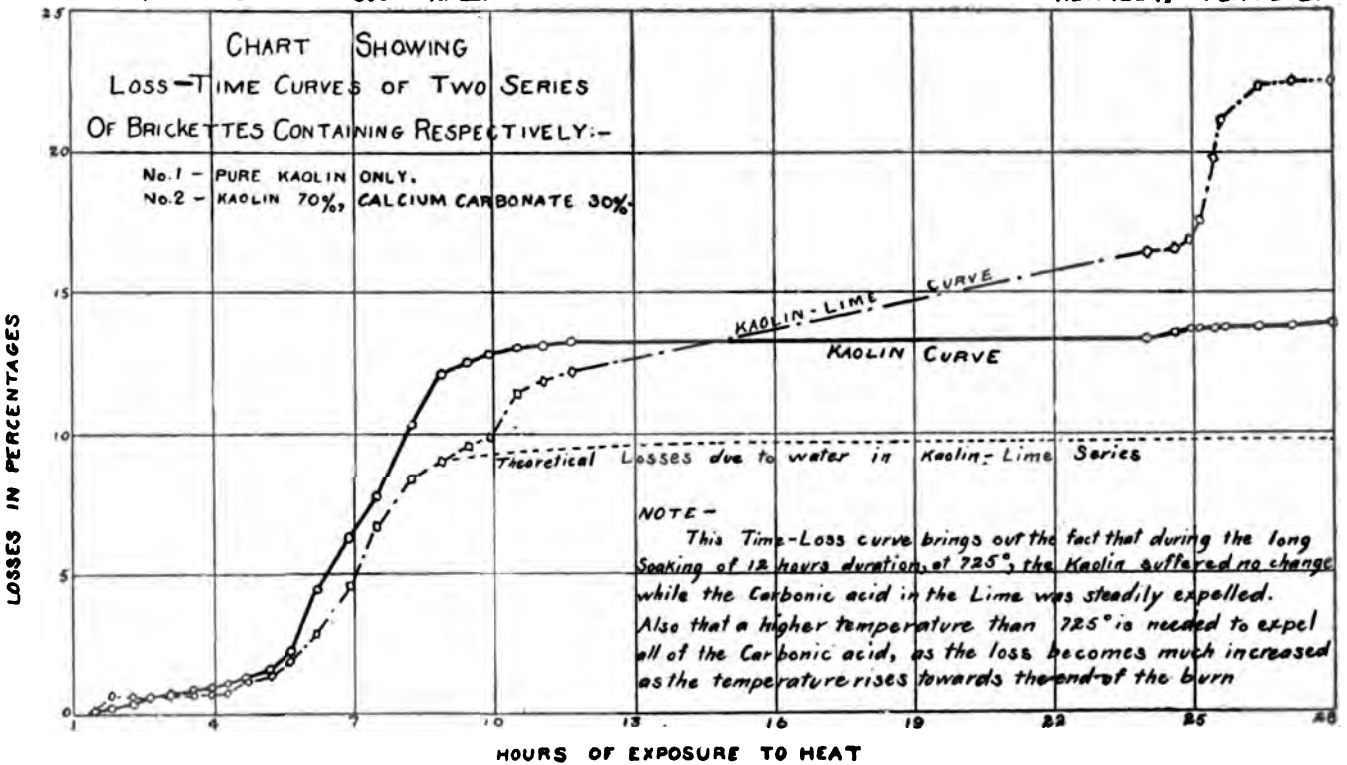


CHART SHOWING
LOSS—TIME CURVES OF TWO SERIES
OF BRICKETTES CONTAINING RESPECTIVELY:—
No. 1 — PURE KAOLIN ONLY.
No. 2 — KAOLIN 70%, CALCIUM CARBONATE 30%.
KAOLIN-LIME CURVE
KAOLIN CURVE
Theoretical Losses due to water in Kaolin-Lime Series
NOTE—
This Time-Loss curve brings out the fact that during the long Soaking of 12 hours duration, at 725°, the Kaolin suffered no change while the Carbonic acid in the Lime was steadily expelled. Also that a higher temperature than 725° is needed to expel all of the Carbonic acid, as the loss becomes much increased as the temperature rises towards the end of the burn
LOSSES IN PERCENTAGES
HOURS OF EXPOSURE TO HEAT
0
5
10
15
20
25
1
4
7
10
13
16
19
22
25
28

TABLE I.

PERCENT LOSS VS. TEMPERATURE.

Percentages of Loss in Kaolin Brickettes.				Percentages of Loss in Kaolin-Lime Brickettes.			
Temp. at Drawing	First Brickette.	Second Brickette.	Average.	Temp. at Drawing	First Brickette.	Second Brickette.	Average.
150	.006	.027	.018	150	.27	.075	.18
200	Chipped	.026	.026	200	.66	665	.66
250	.53	.29	.41	250	.58	.74	.66
300	.69	Chipped	.69	300	.67	1.22	.67
325	.73	.72	.72	325	.45	1.95	.71
350	.75	.75	.75	350	.55	.86	.71
375	.93	.95	.94	375	.77	.66	.71
400	Chipped	1.11	1.11	400	.44	1.13	.77
425	1.43	1.21	1.32	425	1.49	1.28	1.38
450	1.10	2.09	1.59	450	1.68	.985	1.41
475	2.41	2.10	2.25	475	Bad	1.84	1.84
500	4.45	Chipped	4.45	500	3.07	2.51	2.79
525	6.60	6.00	6 30	525	4.53	4.54	4.54
550	Bad	7.75	7.75	550	6.85	6.51	6.68
575	10.30	Chipped	10.30	575	8.20	8.55	8.35
600	12.10	12.10	12.10	600	8.54	9.40	8.97
625	12.60	12.45	12.52	625	9.53	Chipped	9.53
650	12.90	12.74	12.82	650	Chipped	9.90	9.90
675	12.90	13.10	13.00	675	11.40	Chipped	11.40
700	13.10	Chipped	13.10	700	Chipped	11.82	11.82
725	13.25	13.21	13.23	725	12.20	12.20	12.20
725	13.50	13.20	13.30	725	16.40	16.30	16.40
750	13.31	13.70	13.51	750	16.50	Chipped	16.50
775	13.58	13.65	13.61	775	17.10	16.40	16.80
800	Bad	13.65	13.65	800	17.30	17.60	17.45
825	Broken	13.67	13.67	825	20.00	19.52	19.76
850	13.70	13.60	13.70	850	21.20	21.00	21.10
900	Chipped	13.75	13.75	900	22.20	22.40	22.30
950	13.75	Chipped	13.75	950	22.50	22.50	22.50
1000	13.84	Bad	13.84	1000	22.40	22.54	22.50

TABLE No. II.

PERCENT LOSS VS. TIME.

Kaolin Brickettes.	Kaolin-Lime Brickettes.	Hours.	Minutes.
.018	.18	1	25
.026	.66	1	45
.46	.66	2	18
.69	.67	2	40
.72	.71	3	00
.75	.71	3	25
.94	.71	3	50
1.11	.77	4	15
1.32	1.38	4	45
1.59	1.41	5	15
2.55	1.65	5	40
4.45	2.79	6	15
6.30	4.54	6	50
7.75	6.68	7	35
10.30	8.35	8	15
12.10	8.97	8	55
12.52	9.53	9	25
12.82	9.90	9	55
13.00	11.40	10	30
13.10	11.82	11	10
13.23	12.20	11	50

Soaking period of 12 hours duration, during which no gain of temperature occurred.

13.30	16.40	24	00
13.51	16.50	24	40
13.61	16.80	24	55
13.65	17.45	25	10
13.67	19.76	25	25
13.70	21.10	25	40
13.75	22.30	26	10
13.75	22.50	27	20
13.84	22.50	28	00

DISCUSSION.

Mr. W. P. Gates: I have had some experiences in this line myself in our kilns. I used a natural limey clay, and I started drawing my trials much earlier. All were dried at 120° C. and placed in the kilns. On drawing up to about 150° C., the trials showed a decided gain in moisture, the highest amounting to four or five per cent. This gives a pretty good indication that a good deal of fuel could be lost from overdrying the wares. I think many plants dry their ware too much, whereas they could save by underdrying, and letting some of the moisture be taken off in the kiln, as the test showed that it had to be done over again in the kiln in any case.

Mr. Alfred Yates: My experience teaches me that you cannot get the ware dry enough; the dryer you set it, the better results you get.

The Chair: We all know that there is a large amount of water vapor in the kiln in the early stages of burning, and it would seem reasonable that an over-dried clay would re-absorb some of it.

Mr. Yates: I think that good burning requires a low temperature and good circulation in the kiln in the beginning. By this, you get even distribution. If you push it, you will have hot places and cold places, and the hot places will rob the cold places, and prevent equalizing over the whole area. But, if you will hold down the temperature, and keep up the circulation, you will get uniformity from the start.

Mr. W. P. Gates: I was speaking only of muffle kilns. This test of mine was run in a small muffle kiln. I do not know how it would run in an open kiln.

The Chair: The point in the paper is the comparison between clay with lime, and clay without it. Most of us have to deal with lime-bearing clays sometimes, and their behavior is decidedly interesting.

I would like to ask about the condition of the kaolin-lime brickettes at the close of the burn. Was there any difference in hardness?

Professor Orton: The lime-brick or the calcareous mixture was much more porous, of course, at the end of the burn than the kaolin was, because vitrification involving so large an amount as thirty percent of lime would not occur at so low a temperature; so the lime mixture was a good deal softer. If the temperature had gone higher, it would have been harder than the kaolin. So far as I know, the presentation of these facts here is the first time in which a pure clay has been compared with a calcareous clay in the matter of its dehydration curve; and it shows us a very interesting fact, viz., that the calcareous clay is ready for vitrification changes, and for the perfection of its heat reaction at 900°; while the clay-substance is practically ready from six hundred on. There is a stage in the dehydration period in which the gases are coming off the clay rapidly, which can be prolonged three hundred degrees by the presence of carbonate of lime. Two students in the university are now at work on a similar curve, in which ferrous carbonate is being experimented on in connection with kaolinite; the idea being to attempt to prove the point at which CO_2 is being expelled from $FeCO_3$. That is a more practical experiment, because clays containing thirty percent of lime are not commonly used. But clays containing ferrous oxide are in common use, and are especially subject to "black-coring," swelling and numerous other defects. Therefore it seemed to me quite important to show at what point a ferrous carbonate brick was ready for further combination, and so I assigned this problem to these students. This investigation of Mr. Kennedy's seems to me of equal value in a way, though not so directly applicable to conditions in this country.

THE MANUFACTURE OF ARTIFICIAL SAND STONE.

BY

SAMUEL V. PEPPEL, B. Sc., Columbus, O.

During the last five or ten years, occasional newspaper articles written in popular vein have appeared describing a wonderful industry which was to revolutionize the brick business in this country, intimating that brick to answer all purposes were to be made from sand at a cost far below that of ordinary clay brick. Time has not verified these statements, in this country at any rate. Nevertheless, the sand brick industry has a promising future in this country, for the production of ornamental architectural effects, if for no other purpose.

Sand brick, or the "kalksandstein" of the Germans, is a mass of sand particles bound together by either calcium carbonate or calcium hydro-silicate, or mixture of both. Sand brick, with calcium carbonate as the cementing material, or "filler" of the spaces between the particles of sand, have been manufactured for fifty years or more. Sand brick with calcium silicate as the cementing, bonding and filling material, does not date back more than ten or twenty years. In the latter case, there is a chemical combination or reaction between the two constituents forming the brick, which binds the two together in a manner akin to vitrification. The resultant product is similar in many respects to dry press brick, in that vitrification does not penetrate to the core of each particle. In the former this is not the case.

For either product the necessary raw materials are the same—sand or a granular silicate and lime, (either a high calcium lime or a dolomite lime). The method of treatment during the process of manufacture differentiates them into three distinct products, all of which go indiscriminately under the same name.

SAND BRICK WITH EXCLUSIVELY CARBONATE FILLER.

It is obvious that the stone which has carbonate of calcium as a filler or binding material cannot have the same strength as the one in which there is a chemical union of the sand and the calcium or magnesium compound, as the case may be. However, the former can be manufactured at a less cost, and probably fills certain requirements in building construction. Therefore, we will give it brief consideration.

Twenty to forty percent of completely slaked lime is thoroughly mixed with sand and sufficient water to make moulding possible at moderate pressures. The hardening process is carried out in one of three ways:

First. The brick are exposed to the action of the atmosphere for a long time, the caustic lime thus slowly acquiring the needed carbon dioxide ($CaO + CO_2 = CaCO_3$). The necessity for complete slaking is evident, since any unslaked lime would slake in the air and rupture the brick. The time required before they can be used is five to six months, and they attain their maximum strength in about one and one-half years. The hardening here is similar to that of mortar, and like mortar is only superficial. The best lime for this purpose would naturally be a hydraulic lime. Zwick has said that only thirty per cent. of the lime in very old mortars, had been converted into the carbonate.

Second. Hardening in an atmosphere, rich in (CO_2) carbon dioxide, without pressure. This gives the same results as the first process, in less time.

Third. Hardening with (CO_2) carbon dioxide under pressure. It is claimed by some that this means gives a product in which nearly, if not all the calcium hydroxide $Ca(OH)_2$ is converted into carbonate. However, I do not believe that this has been satisfactorily proven.

SAND BRICK WITH PARTLY CARBONATE AND PARTLY SILICATE FILLER.

In this process the treatment up to the hardening process is essentially the same as just described, but the hardening is effected in a warm, moist atmosphere, saturated with CO_2

from the lime kilns, and the heat of slacking lime is used to aid in keeping the temperature up to 60–70 degrees C. A combination of the methods for class 2 and 3, provides for the introduction of CO_2 into the kettles or closed iron cylinders used for hardening in class 3. Under the conditions first cited, the product has a binding material for the most part $CaCO_3$, but some hydrated calcium silicate is probably formed, thus strengthening the bond. In the latter, the binding material in mainly the calcium hydrosilicate, and the amount of calcium carbonate much less than the former.

I am inclined to the belief that here also the superficial hardening prevents anything like a nearly complete change of the $Ca(OH)_2$ in the center of the stone. This of course would be no detriment to this product, or the preceding one, except to give a weaker brick, *provided only* that the surface had reached sufficient hardness and density to prevent all access of water or gases. If this is not obtained, we should expect effloresence as a result of the solubility of the calcium hydrate.

SAND BRICK WITH HYDROSILICATE FILLER.

This brings us to the third class, the only one which I think will ever take any important place in the world's markets. Therefore, we will go into the essential facts more closely.

The Sand.

The same raw materials are used as in class 1 and 2, viz. sand and lime. Considering the sand first, the following facts have been collected.

First. Almost any sand *can* be used, and a fair product produced, if the treatment is properly varied to suit the physical and chemical properties of the sand. Economy in manufacture, however, limits both physical and chemical properties.

Second. A comparatively pure sand is essential to cheap manufacture,—nor should the sand be too coarse. It is probable that for the best results most of it should pass through a screen, 20 meshes to the inch, unless there is a

good gradation from extremely fine to the coarse sizes, in which case perhaps coarser sand could be worked in to advantage. The product would not be as smooth and pleasing to the eye as one made from finer sand.

The object is to obtain a sand which will contain sufficient very fine particles to combine with the lime, and which when pressed will leave the smallest possible interstitial spaces, thus reducing the amount of lime necessary for welding these particles together.

Work done in connection with the cement industry has shown that practically none of the sand coarser than 100 mesh, and only a part of that of 150 mesh enters into the active cement reaction.

As an illustration, suppose we had one sand all of the size of walnuts, and another all of the size of wheat. Enough of the smaller could be mixed in to materially reduce the interstitial space, and yet leave sufficient space for a binding agent.

The finer the material, as a whole, the more difficult will it be to so mix it as to get a coating or film over each particle of sand. On the other hand, the union will be more complete, and the product more pleasing to the eye.

Experiments carried out by Prof. M. Gasenopp in the chemical technical laboratory of the Polytechnic Institute at Riga, the results of which were published in the Thon Industrie Zeitung for October 1900, Vol. 24, p. 1703 and Vol. 25, p. 762, clearly show the necessity of using fine sand for rapid and economic production.

The first set of results were obtained with fat lime as the active agent, and the second with dolomite lime.

In both cases a very pure sharp coarse sand 0.6 to 1.0 mm. in diameter was used, and a fine sand, 0.2 to 0.3 mm. in diameter. The fine sand was somewhat rounded and contained a considerable quantity of soluble silicic acid, therefore it was treated with HCl, and caustic soda solution, and washed before using in the experiments.

Each size of sand was treated with 10 percent and 20 percent of lime respectively, and with 10 and 20 percent of dolomite lime. High pressure steam was used. Both sets

were run for eight hours, one at 5 atmospheres pressure, the other at 10 atmospheres.

The tables of analyses of the resultant products given below speak for themselves. The soluble silicic acid is an index to the extent of the reactions. The methods of analysis are not given. The samples were exposed to the air for two to five weeks, which probably accounts for most of the CO_2 since the $Ca(OH)_2$ was not all converted into the silicate.

[See Tables I and II, pages 161 and 162.]

Considering the above table, we see that the coarse and fine sand was very differently affected under the same conditions, viz.:

TABLE III.

Percentage of Caustic Filler Used	Pressure of Steam in Atmospheres	Percentage of Soluble Silica found by Analysis of Product		
		Kind of Caustic Filler Used	Using Coarse Sand	Using Fine Sand
10	5	Pure Lime	0.43	3.06
		Dolomite	0.75	1.98
	10	Pure Lime	3 33	7.58
		Dolomite	3.14	6.29
20	5	Pure Lime	0.59	3.41
		Dolomite	0.65	....
	10	Pure Lime	2.75	11.14
		Dolomite	1.69	7.03

In every case the reaction is much more complete with the fine sand. This shows clearly the necessity for some fine quartz. The reaction has gone a great deal farther with the fine than the coarse, although the fine sand was not in as favorable physical shape for chemical union as the coarse. If soluble silicic acid were naturally in the sand, it might take the place of fine sand. In each case, the lime was not all combined with SiO_2, that is the reaction was incomplete, as is shown by the presence of uncombined $Ca(OH)_2$. After

TABLE I.

Series A. Brickettes in which pure lime was used. Analyzed after eight hours exposure in steam cylinder.

No.	Mixture	Stean Pressure in Atmosph're	Total CaO	$Ca(OH)_2$ from Total	MgO	Al_2O_3+Fe_2O_3	CO_2	Chemically Combined Water	Soluble SiO_2	Sand	Total
1	90 parts coarse sand..... 10 " lime.....	5	6.50	1.67	....	....	3.45	0.88	0.43	89.10	100.37
2	Same as No. 1.....	10	9.38	1.24	....	0.42	2.54	2.96	3.33	82.00	100.68
3	80 parts coarse sand..... 20 " lime.....	5	16.69	7.16	....	....	4.77	2.78	0.59	74.67	99.50
4	Same as No. 3.....	10	16.59	6.05	...	0.29	3.02	4.60	2.75	72.85	100.10
5	90 parts fine sand..... 10 " lime.....	5	8.74	0.85	....	0.31	3.75	1.79	3.06	82.02	99.67
6	Same as No. 5.....	10	10.10	0.58	....	0.56	2.23	2.55	7.58	76.38	99.40
7	80 parts fine sand..... 20 " lime.....	5	15.81	4.23	....	0.31	5.21	3.90	3.41	70.68	99.32
8	Same as No. 7.....	10	17.67	0.98	....	0.65	3.13	3.59	11.14	63.05	99.42

TABLE II.

Series B. Brickettes in which dolomite lime was used. Analyzed after eight hours exposure in steam cylinder.

No.	Mixture	Steam Pressure in Atmosph're	Total CaO	$Ca(OH)_2$ from Total	MgO	Al_2O_3+ Fe_2O_3	CO_2	Chemically Combined Water	Soluble SiO_2	Sand	Total
1	90 parts coarse sand..... 10 " dolomite..... ...	5	4.98	0.88	3.81	0.23	2.21	2.24	0.75	85.23	99.45
2	Same as No. 1	10	4.49	0.43	3.45	0.96	2.05	2.13	3.14	83.61	99.83
3	80 parts coarse sand..... 20 " dolomite.....	5	8.56	2.53	6.58	0.70	3.38	3.44	0.65	75.80	99.1I
4	Same as No. 3	10	9.97	1.92	7.57	0.95	3.19	4.39	1.69	72.62	100.18
5	90 parts fine sand .... 10 " dolomite.....	5	4.97	0.92	3.75	0.68	2.22	1.82	1.98	84.23	99.65
6	Same as No. 5	10	5.40	0.40	4.07	0.95	1.92	1.88	6.29	79.28	99.79
7	80 parts fine sand..... 10 " dolomite....	10	9.23	0.79	7.00	0.94	1.82	5.74	7.03	67.88	99.64

taking from the chemically combined water that which would go with the $Ca(OH)_2$, the balance of the water gives some striking molecular ratios to the soluble silicic acid. Thus from Table I we have

TABLE IV.

Series A.

Number	1	2	3	4	5	6	7	8
Ratio	3.7	2.66	6.50	3.7	1.7	1.	2.8	1.

Series B.

Number	1	2	3	4	5	6	7	....
Ratio	9 55	2.16	14.4	7.4	2.7	0.93	2.60	....

The ratios 6.5, 7.4, 9.55 and 14.4 seem a little unusual to me. It is notable, however, that with one exception, the high ratios are with the MgO.

These results also show clearly that high pressure is essential to anything like rapid working.

Professor Michaelis is quoted as saying the higher the pressure, the higher the soluble silicic acid, and the solider and denser the resultant brick.

Professor Michaelis has given the physical properties of the sand primary importance, and chemical properties a secondary place. He also says that two-thirds coarse and one-third fine sand give a denser and firmer brick than where the sand is all of one size. 70 per cent. coarse sand, 30 per cent. fine sand, with 5 per cent. added lime gives a good product, having 7 to 8 per cent. porosity. He has also shown that good brick can be made with sand carrying as much as 40 per cent. clay, if sufficient lime is used.

Judging from his figures, I should say for each part of clay present a little less than one part of lime must be added in excess of that which would be needed for the sand itself. While 2 to 3 per cent. of lime is probably all that is necessary to effect a satisfactory bond, in practice more is used, in order to hold the sand together and to facilitate pressing into a form which can be handled.

In conversation with Mr. H. E. Brown, of Coldwater, Michigan, recently, he said 2½ per cent. of kaolin added to a mixture of dolomite lime and sand had given him tensile strength higher, if anything, than brickettes made from same material without the kaolin. On the contrary, it is claimed by others that the clay has a tendency to reduce the hardness, and make the brick more susceptible to weathering action.

From theoretical considerations we might say the presence of silicates in any large quantity would act as a dilutant or inert material with perhaps a few exceptions. Kaolin (Al_2O_3, 2 SiO_2, 2 H_2O) may be converted into anorthite (CaO, Al_2O_3, 2 SiO_2). But we do not know this to be the case. Also anorthite, if present in the sand might take on four molecules more of SiO_2 and five molecules of H_2O as water of hydration and form heulanite (5 H_2O, CaO Al_2O_3, 6 SiO_2), and the acid orthosilicates represented by phenite (H_2O, 2 CaO, Al_2O_3, 3 SiO_2) might take on a molecule of CaO and give up a molecule of H_2O.

It is hardly likely that the normal silicates would become more basic in the presence of large quantities of available silicic acid. It is not likely that the metasilicates, represented by the simplest silicates of calcium and magnesium, enstatite (MgO, SiO_2) and wollastonite (CaO, SiO_2), can do more than promote crystallization of the calcium or magnesium silicates formed, by their mere presence in crystal form.

The silicates containing the alkalies, potassium and sodium, are probably barred on account of yielding soluble salts. From silicates of the heavy metals, the oxides would be thrown out. At any rate, they are too valuable for other purposes to be considered here.

Since under the working conditions, the very resistant quartz is brought into the reaction, the greater part of the complex silicates are very likely to be broken up into more simple forms, in most cases at least yielding inert compounds in part.

At any rate it is evident that when silicates are present in any quantity, more lime will be required, and while it might be possible to make a good brick from a silicate sand, it would not be a good business proposition.

As an instance of the weakening action of inert materials, we might cite the slightly lower crushing strengths as shown in a table given later.* Mr. H. E. Brown also confirmed this observation, finding the tensile strengths less on colored brick than on the same brick without the coloring matter, when treated in exactly the same manner.

Hence it follows that the sand should be comparatively pure, made up of medium coarse with some very fine grains. Sharp sand is preferable so as to present thin sharp corners to promote chemical combination. The sharp sand will also resist change of position or flow of solid particles better than rounded grains. If the sand available contained any large amount of clay it would probably be economy to roughly wash it. And if it did not contain sufficient very fine particles, a portion would have to be ground. This would increase the cost about five to seven cents per 1000 brick produced.

The Lime.

The prime requirement is proper burning. Since dead burning of limestone or dolomite tends to produce a crystalline oxide, which slakes or hydrates with extreme slowness, a portion might remain unslaked and uncombined, and make trouble by future slaking, and possible change of volume and possibly effloresence. It would hardly rupture the brick, since the crystalline oxide is said to hydrate without much change in volume.

Economy, however, indicates the use of the purer limes (lime, dolomitic lime or dolomite lime). The impurity is usually clay or sand, and at the temperature at which limes are burned, the SiO_2 will be combined with the lime, and the Al_2O_3 remain as such, both acting as inert bodies so far as sand brick are concerned.

German practice up to the present time seems to be to use only the fat limes. Ernst Stöffler, an engineer of Zürich, Switzerland, who has published a pamphlet on this industry, says fat limes are the best. Prof. Glasenapps' work, early in 1901, has shown that essentially the same results obtain when working with dolomite lime as with the fat limes.† I see no reason why dolomite lime should not be

*See Table VI. †See Table I.

a desirable material, except that it will require more care and time to properly slake it. We would have the increased fluxing action due to two bases instead of one. Even if the over-burning, which the MgO is bound to suffer before the CaO can be properly burned in a dolomite, should cause it to slake so slowly as to go through the process unchanged, it does not offer the same disadvantages that unslaked CaO would, since the oxide requires 1000 times its weight of water for solution, and the hydrate is practically insoluble. However the unslaked MgO would probably slake when exposed to the hot steam, and in that event might expand and rupture the brick in the hardening cylinder.

The dolomite lime may, however, give a brick which will be slightly lower in fire resisting quality than one made from the fat limes, especially if the amount of lime used is high. If this should be the case, it would not be due to the lower melting point of $MgSiO_3$ but, to the greater activity of two bases acting in unison on an acid.

PROCESSES OF MANUFACTURE PRIOR TO HARDENING.

Present practice adopts one of four methods prior to hardening: 1st. The wet slaking process. 2nd. The dry slaking process. 3rd. The acid slaking process. 4th. The quick lime or oxide process.

First Method.

This process consists in slaking the lime to a fat putty, either by hand or by mechanical aids, and then mixing in the desired proportion of sand and water, either in a wet pan or a large, slow moving pug mill. From here it is carried to bins over the press, allowed to stand a short time to ripen and then pressed.

With properly burned fat limes, the proper amount of water and sufficient agitation, this is accomplished very quickly and thoroughly. The heat given up by the union of CaO and H_2O is sufficient to generate steam in the minute pores of the amorphous oxide, and thus break it up into the smallest possible particles, constantly and rapidly exposing new and fresh surfaces to the hot water. CaO on hydration

gives up 270 heat units. Under these conditions the CaO seems to form a hydrate, carrying more than one molecule of water, the excess being locked in so loosely that a little excess of heat, as would be the case with too little water, would prevent its formation, or break it up if formed. Water acts as an equalizer; not enough allows it to get too hot, and too much cools it too low for the best results.

Walters found that the best slaking of high calcium lime gave an increase of volume of 3½, air slaking 2½, and slaking with a large quantity of water $1\frac{7}{10}$. This increase in volume gives it greater spreading or enveloping power.

With the dolomites more time is required for proper slaking, for two reasons: The MgO is somewhat overburned, and second, the heat units evolved by MgO on hydration is much less than that for the hydration of CaO. Both act in the direction of retardation.

The process can be much improved and quickened by the use of hot water or the application of external heat.

It is claimed by many that the magnesian limes never develop the same degree of plasticity as the fat limes. This may in part be due to imperfect slaking.

Second Method.

This differs from the preceding only in this, that the lime is slaked with just enough water so that the heat of the chemical reaction shall just dry the finished hydrate. The hydration is incomplete in most cases. There are a number of patent machines for doing this, both in this country and in Europe. Some are closed tight, and the slaking done under pressure with water alone, others are treated with both water and steam.

In another type, water is added to the lime in a closed vessel, and heat applied externally by means of a steam jacket to the container.

Such hydrates of this class as I have examined, have been found to be very imperfectly hydrated. However, none were examined when water was added and steam supplied to keep the mixture hot, or where external heat was used.

Mr. H. E. Brown claims that dolomite lime hydrated under the conditions last mentioned in a steam jacketed

machine, showed on chemical analysis complete hydration, with a three-fold increase in volume, and yet a dry product.

Third Method.

Five to ten per cent. of a solution of commercial hydrochloric acid—19 degrees B., is added to the lime after slaking has begun. This is the German patent of Peter Kleber. (No. 103777.) This acid is worth about one to one and one-half cents per pound. In this process the preliminary treatment only carries the hydration to from one-third to one-fifth of completion.

This of course gives a small amount of $CaCl_2$. The affinity of this compound for water is well known. It is also very active in attacking silicates, as is illustrated by its action in the nascent condition in the J. Lawrence Smith fusion for the determination of alkalies.

The introduction of the acid must be objectionable at any rate, since if the $CaCl_2$ remains as such, it would be soluble later, and give trouble. If it is decomposed, and $Ca\,SiO_3$ formed, the acid liberated in the warm moist atmosphere must certainly attack the metal cylinders unless it is glazed with a silicate of some sort or lead lined.

$$CaCl_2+SiO_2+H_2O=Ca\,SiO_3+2\,HCl.$$

It is claimed by Kleber, that the HCl accelerates the slaking and shortens the time needed in the hardening vessel. The introduction of $CaCl_2$ is not so satisfactory as the use of the acid. While there are a number of plants operating under this patent, I do not believe that much acid is now used.

Fourth Method.

The quick lime or dry oxide of calcuim is mixed with the sand, and just enough water to make it possible to work it in the press. The water is added in two portions, with a short interval of time between additions. The product then goes to the press.

This certainly is the most rational and rapid method, if as good a product can be produced as by other processes. This process probably requires a little more lime than the others in order to get the mixture in proper condition to

mold. Where the cost of lime is very high, this would be an item. But much may be done to reduce this lime by mixing with it a portion of extremely fine ground sand.

If lime hydrates with steam under pressure in a crystalline form without change of volume, as Walter's experiment in hydrating lime in a glass tube with steam seems to show, the hydration during hardening would not in any way injure the brick.

Statements made to me by Mr. Brown, however, indicate that for dolomite there is a large increase in volume. He used water and steam both. My own experience points to the fact that both CaO and MgO will expand when treated with steam. If such be the case the fourth method would not be feasible.

It is also possible that the hydro-silicate of calcium may form direct from the CaO without change in volume. If this were the case, we should work with as little water as possible to get the denser brick.

HARDENING PROCESSES.

The hardening is accomplished in one of two ways, either by low pressure steam and long exposures, or high pressure steam and short exposures. In the first case, the pressure used is about two atmospheres, giving a temperature of 120 to 125 degrees C, and the duration of the exposure is about seventy-two hours; in the second case, the steam pressure is from eight to ten atmospheres, giving temperatures around 185 degrees C, and the exposure lasts for a few hours only, six to ten hours being sufficient in most cases.

The letters patent on this latter and most important procedure were taken out by Professor Michaelis early in the eighties, and permitted by him to lapse without commercial application.

I have already mentioned a modification of the hardening process in connection with class No. 2, in which CO_2 is introduced either with the steam or after it. If this method is used at all, it is usually when working with low pressure steam.

According to Stöffler, the quantity of lime used in any of the methods varies from 5 to 10 per cent. And the moisture above that used in hydration, from 7 to 9 per cent. The nature or the make up of the sand, whether quartz or feldspathic or clayey, will determine the amount of both lime and water desirable.

From the foregoing statements you can readily conceive that it would be entirely feasible to take the sand from the bank and have a brick ready for market in less than a day of twenty-four hours, or if you include the burning of the lime, the entire process from the raw limestone and sand to the finished product need not consume more than thirty-six hours.

QUALITY OF THE PRODUCT.

Concerning the quality of this new artificial stone, the question arises in the mind of each of us: "Is it of any value to us?" "Will it fulfill our requirements for a safe and desirable building material?" I think it will, and offer the following as evidence of its quality:

The sample before you is one made in Germany, by the Stahl and Eisen Actien-Gesellschaft, Hoerde, Germany, from materials which you will see were not the most desirable. An analysis by rapid methods gave;

84 per cent.		SiO_2 (sand and Sol. SiO_2).
2	"	$Fe_2O_3+Al_2O_3$
7	"	CaO
	"	MgO low.
93. per cent.		
7.	"	Balance is made up of MgO, H_2O and CO_2 and Alkalies.

When three grams were ground fine and treated with a large amount of water, only a trace of Ca was found in the water.

The water absorbed on standing over night is 9 per cent. The question naturally is, "Will it stand weather?" The small piece was soaked in water, frozen and thawed three times. It was thawed out the last time on the laboratory hot plate at about 250 degrees C. I could observe no

evidence of damage by frost, except when it froze fast to a sandstone window-sill, and broke out a little when I wrenched it loose. After thawing out, and while still wet, I set it up and turned a blast lamp directly onto one small piece of it, and brought it up rapidly to a bright red heat, and held it for fifteen minutes, then plunged it into cold water. This last proceedure destroyed the bond, as you can see by rubbing the brown spot on the brick. A block sawed from this brick and crushed, showed a crushing strength of 4210 pounds to the square inch.

Further in a paper given by Herr G. Beil, of Aschersleben, at a meeting of the Kalksandstein society in Berlin, March, 1901, the following results are given as coming from The Köenigl. Mechan–technichen Versuchsanstalt, at Charlottenburg, Germany.

Seventy brick, representing 7 different products, were soaked in water at 12°-15° C., frozen for four hours then placed in warm water three hours. All were pronounced sound at the end.

Also the following list of tabulated results (published in Vol. 25 Thon-Ind. Zeit. 575):

TABLE V.

Number of Test		Absorption	Crushing Strength, in pounds per square-inch		
		Amount of water taken up by dried sample, after 125 hrs. soak, in percents of dry weight.	Water soaked Brick	Dry Brick	After Freezing
1	High Pressure Hardening	12	1903	1704	2229
2		14	2073	2215	2300
3		9	3933	4189	4260
4		10.6	1846	1732	2187
5		——	——	3710	3238
6	Low Pressure Hardening	——	——	850	1335
6a		——	——	1353	1747
7		18.3	1335	1193	1562

This shows that the crushing strength is not, in most cases, very materially decreased by freezing. Crushing wet also produces a slight decrease.

In the Thon-Ind. Zeit. Vol. 25, 1660, the following tabulated results are to be found:

Swedish brick, tested at Gothenberg by same method as in above Table V.

TABLE VI.

Kind of Brick (All Lime-Sand)	Specific Gravity in Powder Form	Actual Specific Gravity	Percentage Absorption	Porosity in Percents of Volume	Number of Brick Tested
1. Smooth, gray-white	2.51	1.74	13.1	30.5	10
2. Yellow-brown to red	2.46	2.10	13.6	14.6	10
3. Smooth, gray-white	—	—	—	—	4
4. Brown-red	—	—	—	—	4

	CRUSHING STRENGTH								
	Dry			Wet			Frozen Repeatedly		
	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.
1. Same as above	17.32	12.21	15.48	15.90	10.37	13.21	14.91	7.24	11.98
2. Same as above	14.63	7.53	11.08	14.48	6.36	9.80	11 64	9.23	10.08
3. Same as above	27.41	15.19	22.86						
4. Same as above	21.58	12.35	15.62						

I do not know the process of manufacture.

This again shows a slight decrease in crushing strength after repeated freezing.

Mr. Brown has obtained tensile strengths as high as 650 pounds, but gives as an approximate average for ten per cent. of the hydrates 400 to 425 pounds. He thinks the maximum tensile strength is obtained with very fine sand and about fifteen to twenty per cent. of the hydrated dolomite lime equivalent to nine to thirteen per cent. of the oxides.

A cube 2x2x2 made by Mr. Brown from finely ground sand and dolomite lime, gave a crushing strength of 5730 pounds to the square inch when crushed in the mechanical laboratories at Ohio State University.

The following is a copy of tests made on brick manufactured by H. Huennekes:

Tests of Lime—Sand Building Brick.

Made for A. G. Roenick, 408 Wood Street, Pittsburgh, Pa., by the Pittsburgh Testing Laboratory, October 17, 1901:

Crushing Strength Per Square Inch	Absorption Test 45 Hrs. in Water.	Absorption After Freezing Test	Crushing Strength Per Square Inch After Freezing
3518 lbs.	11.6 % gain	12.4 % gain	* 4187 lbs.
4162 lbs.	8.57 % gain	8.8 % gain	* 5202 lbs.
3859 lbs.	11.3 % gain	11.4 % gain	

* Bricks did not crush at this load, but showed only slight cracks at an actual load of 105,000 pounds, the limit of the machine.

Half bricks were used in absorption tests; these bricks were dried thoroughly before immersing in water. After remaining in water forty-five hours, bricks were frozen four hours at a temperature of 14° Fahr., and then thawed in warm water twelve hours, frozen again at a temperature of 9° Fahr. for a period of three and one-third hour, thawed in hot water three hours, frozen at a temperature of 12° Fahr. for three and one third hours, and finally thawed in hot water for twelve hours. Final absorption test made and then bricks were again thoroughly dried. The bricks showed no signs of cracking or disintegration.

From the foregoing, it is evident that any of the high pressure steam process stones have strength to meet all ordinary requirements for building purposes. In Germany a great many of the fire inspectors have pronounced them a safe material from their standpoint, and a good many sand-brick have gone into municipal and government buildings over there. The railroads are using them also.

I have taken the following from Thon-Ind. Zeit. V. 25, 1822:

A small oven was built of brick made by high pressure steam hardening, by Guthemann, of Niederlehme, some whole and some bats. It was fired by wood up to a temperature of 1100 degrees C, measured both by Seger cones and

by electric pyrometer. The stones were not injured in any way by this process, and further showed that they were excellent non-conductors of heat.

Later, by means of a blast, the furnace was brought to a glowing heat and the hose turned directly on to the hottest part with a full stream. This cracked off a few corners. When thoroughly cooled it was found that the bond had been destroyed for a depth of 3 cm.= ¾ in., so that part could be scraped off with a knife.

Mr. G. Beil, Thon-Ind. Zeit, 25, 577, cites the following good qualities:

1. Great hardness.

2 and 3. Regularity of form which permits of more rapid and consequently cheaper building.

4. The beauty of the structure.

5. Outside dressing or painting unnecessary.

6. Inside plastering unnecessary in many cases.

7. No insulation necessary.

8. It is cheaper than cut stone or terra-cotta.

9. Cheaper than brick. The costs in Berlin are: Ten to twenty marks per 1000, for sand-lime brick; seventeen to twenty-five marks, per 1000, for face brick. Of course eight and nine represent German conditions, not American ones.

MECHANICAL EQUIPMENT FOR MANUFACTURE.

As has been intimated in previous statements, the machinery in a general way consists of apparatus for either wet or dry slaking, conveying machinery, mixing machines, presses, trucks, hardening boilers and machinery for generating power.

There are a large number of patented machines for the dry slaking of lime, both in this country and in Europe. I know of but one, however, for wet slaking. In this latter process, the lime is slaked with sufficient water to make a good putty, and after maturing in this condition, it is dried by artificial heat and ground.

Since in the various patented processes for making sand brick, the only claim which, in most cases, could be con-

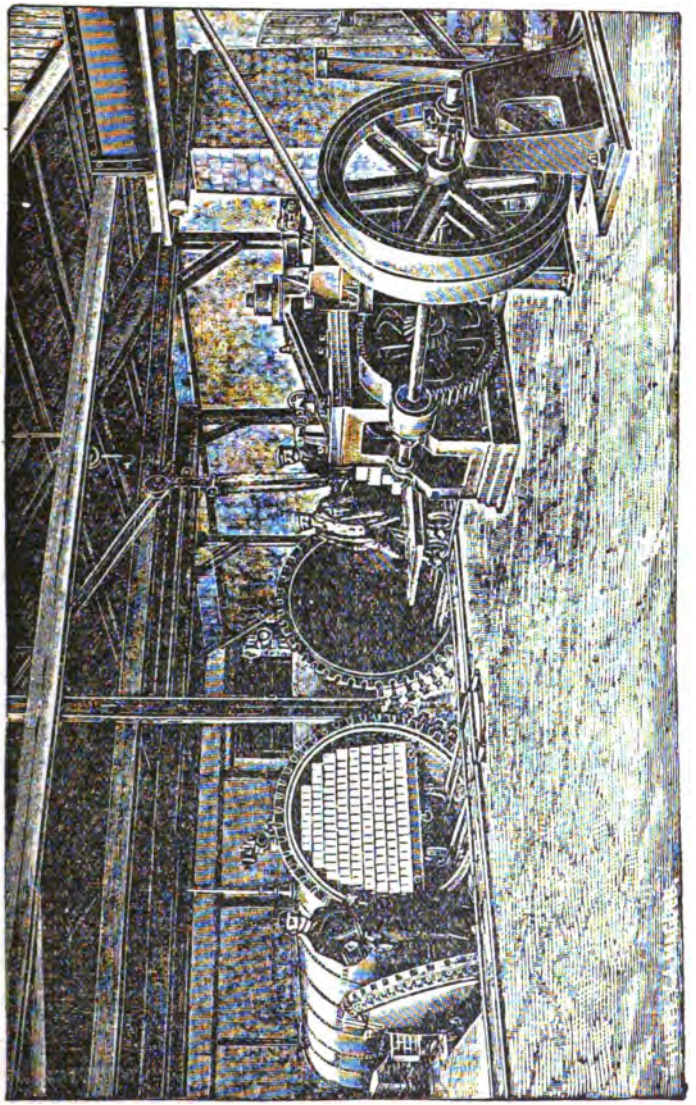


Figure 1.

tested, is that for some special machine or method of slaking lime to a dry powder. I will briefly mention a few of them:

Olschewsky, in one of his German patents, uses a cylinder capable of resisting strong pressure, so mounted that it can be rotated, and receive a supply of water at the same time. The cylinder is filled one-half full of CaO, and sealed up. Rotation and the introduction of water begins: rotates one-fourth to one hour, and allows to stand two or three hours. Thirty to thirty-five parts of water are used to one hundred parts of lime.

From what I have seen in this country, I feel safe in saying such a machine will give imperfect hydration.

Another of Olschewsky's patents, German No. 82785, consists in the the building of a tight metal box in the bottom of the trucks for carrying the brick in the hardening chamber. This is provided with a stirrer, a shaft with peddles. Water and steam are introduced. The heat generated serves to dry the green brick. This wagon then is run into the hardening chamber, and you always have one day's supply of slaked lime ahead.

The German patent, No. 82785, was recently annulled.

This device or proceedure is patented in England, 1900, No. 1705, and in the United States, Dec. 1900, No. 663459.*

There are several other American patents using various forms of agitators.

Another we might mention is that of A. G. Schwarz, of Zürich, Switzerland, in which the machine is heated by steam pipes and the hydrate is dried by producing a vacuum, as all excess of water is pumped out as vapor. This machine is illustrated and described in "Silico-Calcareous Sandstones," by Ernst Stöffler.

Mr. H. E. Brown holds patents which claim for dolomite lime everything which is claimed by the Germans for lime.

It is apparent that the best conveying machines will be those which will move the material with as little friction between the conveyor and sand as possible, on account of the latter's strong abrasive properties. Broad continuous

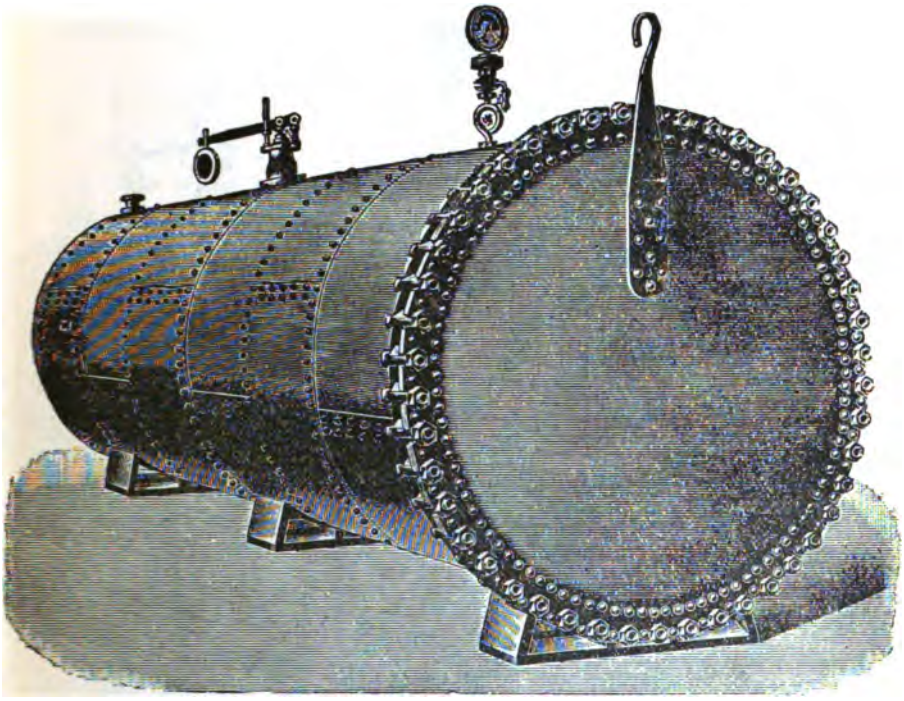
*See Patent Office Gazette, Dec., 1900, page 1946.

belts, where feasible, or heavy chain bucket conveyors are perhaps best adapted to this purpose, where dump cars are not desirable.

If screws or auger machines are used for conveying or mixing, they should be of large diameter and capacity, and geared to move slowly.

Edge runners are much used in mixing and grinding.

Figure 11.



The trucks are usually built low and made to carry from 1,000 to 1,400 of brick of American size, or fewer of German size. They usually carry thirteen to sixteen courses.

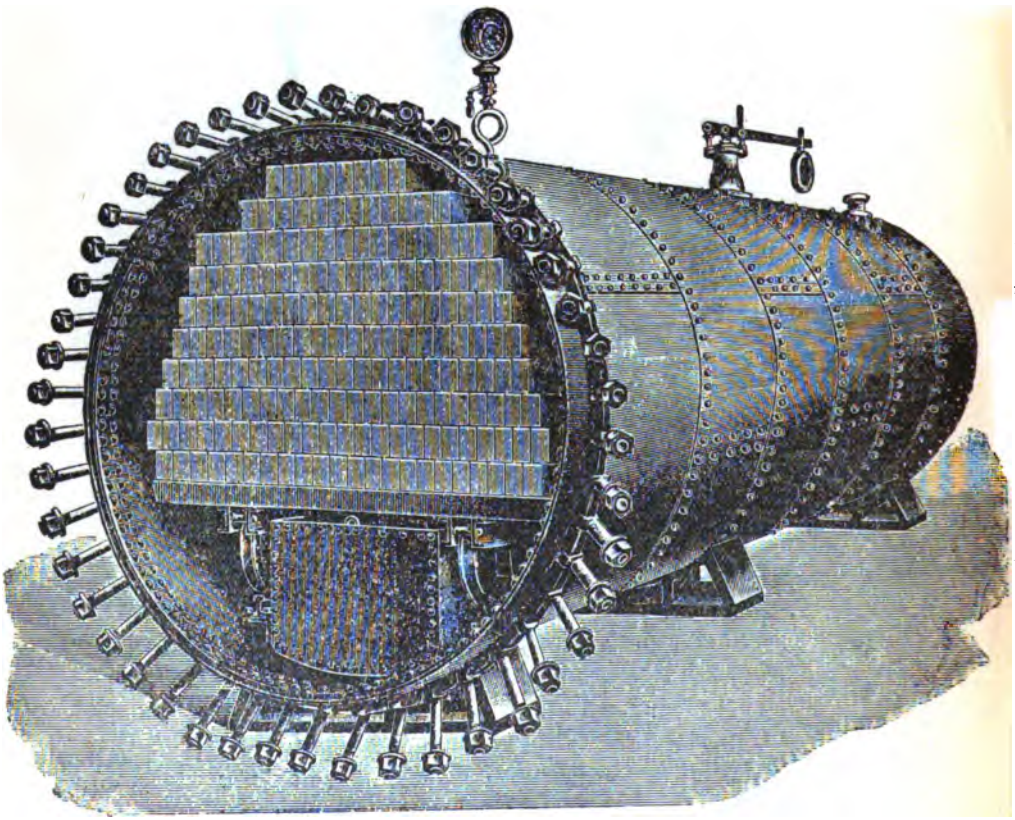
The hardening boilers are made of iron or steel, and constructed to stand for high pressure — 150 to 200 pounds pressure to the square inch. They should always be bricked in to prevent rodiation and excessive condensation of steam. They usually have one head movable and bolted on. This head is handled by a small crane.

The customary size is six to seven feet in diameter by fifty to 60 feet in length.

Figure II shows a hardening boiler closed.

Figure III, a similar boiler open and charged ready for steam as soon as the head is bolted into place. This plate also shows a container for slaking lime, under the floor of the truck.

Figure III.



There are a number of mixing machines in use—wet pans, auger machines, pug mills, &c. One of rather unique type is that of A. G. Schwarz. It consists of a drum with a mixing arm. A cut of this machine may be seen in "Silico-Calcareous Sandstones." It is arranged so as to remove

moisture by producing a vacuum, and then add a definite amount of water, working on definite amount of material. It is necessarily intermittent in its action.

Although an expensive way of operating, it has some things in its favor, since the amount of water cannot vary widely for any given mixture. The amount of water which is best must be determined for each plant.

When once this is determined the Schwarz machine offers a means of always getting that amount into the material.

COST OF PLANTS AND PRODUCTION.

Ernst Stöffler, of Zürich, in his pamphlet gives the following for a plant using quick lime. The cost of a plant and of production will not vary widely from this for any of the others.

PLANT, 40,000 DAILY CAPACITY.

Land and Buildings.

About 5000 m_2 of land, of which about 1500 m_2 is built over, machine masonry, chimney, tracks silos, etc....	$24800 00

Machinery Plant.

A.	Preparation of the lime: One stonebreaker, conveying screw with elevator, one ball-mill, tilting waggons	2332 80
B & B_1	Mixing machines: Two elevators, two intermittent-charge mixing machines, wagons, two continuous mixing machines, moistening apparatus, two elevators	4082 40
C.	Press room: Two endless belt conveyors, six mixing machines, six presses, wagons, moveable platform	14774 40
D.	Hardening room: Three hardening boilers with pipes	7776 00
E.	Boiler: Two steam boilers	5054 40
F.	Engine house: Steam engine.	2916 00
G.	Shafting and pulleys	2916 00
H.	Lighting	1166 40
I.	Unforseen, sundries	2721 60
		$68040 00

To bring the machinery to this country, excluding hardening room and power machinery, would cost an import duty of 45 per cent. of the German valuation; therefore, we have

A	$ 2,332.80
B	4,082.40
C	14,774.40
Total	$ 21,189.60

Duty = $9,535.03 — and freight and transfer charges will bring this up to $10,000, so we would have such a plant costing in this country about $78,000 equipped with German machinery. This alone would add eight cents per thousand to the cost of production.

Figures for cost of production B:

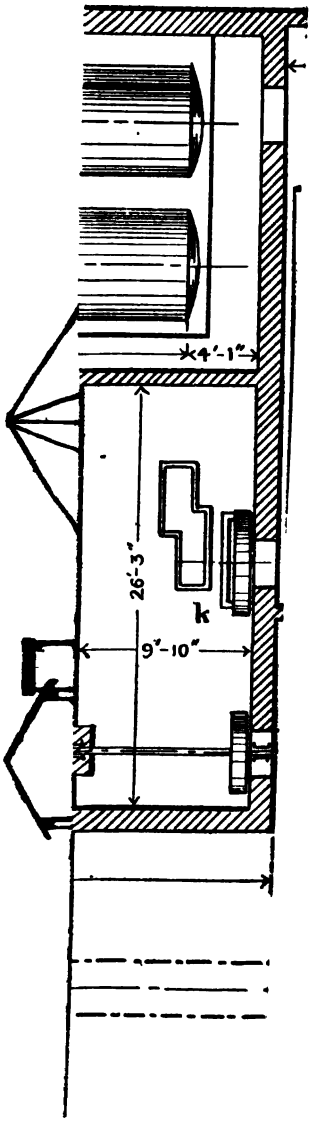
AMERICAN CONDITIONS OF PRODUCTION.

Sand — 157 cubic yards @ 7 cents.....	$ 11 00
Lime — 13 cubic yards = 11 tons @ 4.00 per ton — ($3.00 if you own your own stone and burn it yourself)..	44 00
Coal — 3 tons @ 2.25.....	6 75
Repairs.....	5 00
Oils, etc.	3 00
Forty workmen @ 1.35.....	54 00
Foreman @ 2.50.....	2 50
Office expenses.....	20 00
Depreciation and interest, 10 %.....	20 00
	166 25
Selling expenses — about 10%.....	16 00
	182 25

According to the estimate, the brick could be put on the market without profit at 4.55 per 1,000 when machinery is bought in Germany and with a plant costing $80,000. However, there is no necessity of importing machinery.

A plant with American machinery will cost about, as follows:

A liberal estimate for land and buildings, I think, would fall well within.....	$15,000 00
One wet pan (30 HP).....	1,000 00
One Ball mill (5 HP) or other arrangement.....	500 00
Two presses.....	4,400 00
Two pug mills (20 HP).....	800 00
Conveying machinery.....	6,000 00
Shafting and belting	3,000 00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One 100-HP Corliss engine	2,500 00
Two 100-HP boilers	2,000 00
One 25-HP boiler	300 00
Four hardening boilers, 7 diameter by 60 long	8,000 00
Placing same and insulating same	1,000 00
Pipes for preliminary heating	1,000 00
Railroad tracks and unforeseen expenses	4,500 00
	$50,000 00

I think a plant satisfactory in every way could be installed for $50,000, and brick produced for $4.00 per M., ready for market, if the plant was favorably located.

At any rate, it is safe to say that this product can be put on the market as cheap as pressed brick, since the losses in manufacture, in a plant in good running order, do not exceed two per cent.

I have purposely made estimate of cost of plant rather high, since with any young industry there is more or less delay and expense in getting everything properly adjusted. The time consumed and amount of this expense of course will depend on the ability of the men in charge.

I have used the figures given by Mr. Stöffler since they seemed to represent about the average of several estimates which were available for comparison.

In the back of Mr. Stöffler's book, "Silico–Calcareous Sandstones," are a number of plates giving plans of various sand brick plants.

The plans here published were furnished by Mr. Fred C. Jenkins, of Hamburg, Germany.

The plan of plant here given is not one which would meet our requirements, but is given to show what sort of a place the sand brick plants now operating are. The illustrations were copied from advertising matter, because they show types now in use.

I take this opportunity to acknowledge my indebtedness to the following persons for much valuable information: Stahl and Eisen Actein Gesellshaft, Höerde, Germany; C. Luke, Eilemburg, Germany; Fred C. Jenkens, Hamburg, Germany; Ernst Stöffler, Zürich, Switzerland; Sandsteinziegelei-Patente Schwarz, A. G., Zürich, Switzerland; Brüch Kretschel & Co., Osnabruck, Germany.

One plant has been started in this country and two more are contemplated soon.

DISCUSSION.

The Chair: We have with us this morning, Mr. H. E. Brown, of Coldwater, Mich., a cement chemist, who is interested in the sand brick industry, and who has taken out patents, I believe, in connection with the same.

Mr. H. E. Brown: I have with me this morning, as you will see on the table, some results of investigation of the sand brick question, as carried out in the laboratory of the Michigan Portland Cement Company, and I will be glad to have you examine them. They were not made under ideal conditions, however, because all had to be done by hand methods, and these methods could not be just exactly the duplicate of those which would be carried on with machinery.

I will say in regard to Mr. Peppel's paper, and concerning cost of installation of a plant, that I think he has been very liberal in his estimates of cost of installation of plants. From some few figures which I have been able to obtain from builders of machines, I think that he has figured very liberally, and a person would have quite a good working capital left, if he had fifty thousand dollars in his pocket with which to build a plant to make 40,000 brick per day under my patents.

You will notice in the tensile tests of brickettes, mentioned in this paper, that they were as high as 600 pounds to the square inch. This is stronger than cement brickettes. Brickettes made of one part of cement and three of sand on a seven-day test, usually give 300 pounds per square inch; on twenty days' test, it usually runs up to 400, 450 or 500 pounds. A brick made from one part of dolomitic lime and nine parts of sand, thirty-six hours after being made, will, when submitted to tensile test, give about a uniform test of 400 to 450 pounds to the square inch. I will be pleased to answer any questions as to the details of the business, as far as I am able. My patents have not as yet been published, and I am now working to secure a better knowledge of the actual working conditions to be met and overcome.

BACTERIAL GROWTH AS A FACTOR IN AGEING CLAY MIXTURES.

BY

EDWARD C. STOVER, TRENTON, N. J.

The ageing of clay mixtures before use, has been practiced in the pottery industry so long, that even the histories of the art do not shed light on when it was first begun. It is quite likely that the benefits in "ageing" mixtures, like numerous other improvements, was first discovered accidentally, centuries ago; perchance, some sanguine clay worker had his fond hopes blasted by his inability to use some new mixture which he had put together with the hope of improving his wares, and after numerous fruitless efforts, was compelled to cast the whole batch aside, and return to his old mixtures. Months, and perhaps years may have elapsed, when a series of conditions arose which prevented him from getting his old supply of materials, and, driven by a pressing demand for his product, he, as a last resort, decided to make another effort with the old abandoned batch, and was astonished to find that it not only worked better and easier, but that he had less loss in drying and firing than ever before in his experience, and thus the discovery may have been made. It has been said that the ancient Chinese potters worked up the mixtures prepared by their forefathers, and and that they in turn put up and stored away the mixtures for the next generation.

Be this true or not, we all know from our own experience, that the proper ageing of clay body mixtures greatly increases plasticity, and lessens cracking in drying and firing, and the questions naturally arise: "What has taken place during this process of ageing, and what has caused this change in the mixture?" The answer to the first is apparent: When a mixture of the different materials is first made, certain affinities or attractions remain unsatisfied, so to speak; they are attracting or in search of other affinities, which, in turn, are also attracted towards them, but owing

to their location in the mixture they are unable to unite, and thus the batch is truly a mixture and not a compound. Proper ageing, however, results in a re-adjustment, as it were, and the searching affinities have found their mates and become satisfied, and the batch has become a compound.

What has caused the change, or made it possible for the mixture to become a compound, is the perplexing question. Having given this subject considerable thought, and after making numerous experiments, I find this change is a fermentation caused by bacterial growth. During the last few years, these infinitesimally small organism have engaged the attention of some of the most eminent scientists, and by their discoveries, particularly in the medical field, our knowledge of the world has been wonderfully modified and enlarged. Quite a number of species of bacteria have been found in water alone, while those found in clays and soils number hundreds.

To get anything like definite data on the subject in hand would require months of constant labor in bacteria culture, and the examination of all of our materials used, and the elimination and classification of the various groups, and the determination of the effects on each of nitrogen, hydrogen and the other common gases and elements generally, and how each class would thrive under different degrees of heat and cold, and how each class would change at different intervals of time, all of which would require more time and attention than could be given by a potter, with his time fully employed each day.

As is well known and established by the works of Pasteur, Koch, Cohn, Tyndall, and many others, fermentation is the result of bacterial growth. Like the investigation of almost any scientific point, that may seem a very small one, and obscure at first glance, this subject on investigation, looms up with innumerable phases, till one inexperienced in bacteriological work is at a loss to know where to begin.

In making examination, I was assisted by I. M. Shepherd, A. M., M. D., a graduate of the University of Pennsylvania, and A. W. Atkinson, A. B., M. D., a graduate of the

Hahnemann College, Philadelphia, Pa., both experienced in bacteriological work, and to both of whom I am indebted for much information and literature on the subject.

The predominating species found in clay mixture is believed to be bacillus sulphureum, also called proteus sulphureus; they give off sulphuretted hydrogen gas (H_2S). Their development is facilitated by a temperature of 37 degrees C, to 38 degrees C, which is equal to 98.6 degrees to 100.4 degrees F.

In a fresh mixture allowed to stand in the slip state, the fermentation will be completed in from six to twelve weeks, according to the temperature and other conditions, without inoculation, but by inoculation, the time is cut down to two to four weeks under the same conditions.

As the ageing of body mixtures is mostly done in damp cellars when clay is fresh from the presses, it could readily be thoroughly mixed and inoculated by pugging and adding at the top of the pug mill a small stream of well inoculated and seasoned slip of the same mixture. Then pack away in the damp cellar, and keep constantly warm and damp, but not moved. In this way the full benefits of a proper ageing could be accomplished in a very short space of time, the many economies of which are obvious.

DISCUSSION.

Mr. Stover: I want to say that Mr. Paul Gates has a microscope and some fine slides in his room, of which you can get some idea from the micro-photographs which I have passed around. Plate A. shows in the center a long snake-like bacterium about ready to break apart. Plate B. shows a number of very small parallelograms clustered together, evidently parts of a large one that has just separated. Plate C. shows a number of the small parallelograms scattered among the irregular dark marks. The former are bacteria, the latter are the particles of clay substance. Mr. Gates extends an invitation to all who may wish, to call at his room and see these little fellows in motion; and I must say that my own ideas were very much less clear until I saw what he has to show. I made several efforts to get some-

thing tangible to bring to this meeting, but did not succeed, and I feel credit is due to Mr. Gates for making this paper so much more real than it would otherwise be.

Mr. F. W. Walker: I am more and more impressed with the great modesty of our members, and those who have had the greatest experience are the most modest. This point touches the white-ware potter particularly. The brickmaker weathers his clay in the open air, under conditions which are not so favorable to the growth of bacteria. We sometimes think our cellars should be kept clean, but Mr. Stover has pointed out that to be useful, they should purposely be kept infected with bacterial growth all the time.

Mr. S. M. Ferguson: The ageing of clays is a big thing in reducing potters' losses. Some white ware potters mix from day to day, and have great losses; while others mix two months ahead, and thus reduce their losses wonderfully.

Mr. D. C. Thomas: Sometime ago our firm began using chemicals to wash our press cloths out. I was rather surprised, and asked why it was done. The reply was that there was something in the sacks that destroyed them. I commenced making inquiries, and learned that a chemist for some southern clay firm had reported that he had found bacteria in the clays. And that the bacteria were what was destroying the sacks. I began then to think that perhaps this question might have some bearing upon the ageing of clays. I had no proof, but I had my own opinion.

Mr. S. M. Ferguson: At one time, we had about twelve presses of slip, which were left in the agitator for about a month, owing to a sudden shut-down. When we started, we pumped it into the presses, and it destroyed a bran new set of sacks entirely. They were no good at all after pumping those twelve presses of slip. The sacks were practically gone. The only way we could account for it was from the ageing of the slip. It was blue when it came from the press, but it should been a white mixture. It was an excellent clay. The hands all said the body had been changed. I said it had not, but they insisted it had, for the clay was now blue, and smelled bad; in fact, the clay was practically

rotten. But in the evening of the first day, every person was telling me the results of the day's work. Every one said that they had better results, less cracking, &c., and the clay worked extra well. But it was hard on the press cloths.

Mr. John Stamm: We are of the same opinion as Mr. Ferguson. We always try to keep the cellar full of clay. The older it is the better, and the less loss we have. That is the way we find it.

Mr. Stover: There is one point I want to make—having been convinced that the little creature is there, we want to use him to the best advantage. We all know that if we get a six month's supply of clay ahead, and there is any mistake, it is a good deal of loss. But if we could accomplish the desired results by adding a well inoculated slip in the pug mill, and thus get the clay in the proper condition to promote the most rapid fermentation, I believe in two or three weeks' time we could get a body which would be as mellow and ripe, so to speak, as we now get in several months' time.

Mr. S. G. Burt: We have had this topic up for discussion sometime ago in the society. The idea, as Seger expresses it, is that as your clay stands in ageing, it gradually developes an acid. I do not think he went so far as to get to the point of bacterial growth. What he clearly claims, and I think proves, is that it is this development of acid which actually brings about plasticity. He gives examples of it by testing clay with a little sodium hydroxide in it, which would not settle for a long time, and when it did, was very weak or tender. He prepared another, I think, with acetic acid, which so increased the toughness of the clay as to make it stand working extra well.

If these animaculæ are going to produce sulphuretted hydrogen, they will have to draw the sulphur from the pyrites present. It does not throw any light on the acid formation. I don't know what organic acid they could create, or whether there is any direct connection between the formation of acid, and the bacteria; but Seger has shown that the formation of an organic acid is the direct cause of increased plasticity in the clay, which is a great benefit to the potter. Mr. Stover and Mr. Ferguson's contributions seem to connect this acid formation with bacterial life.

Mr. W. D. Gates: I hate to differ with my learned brothers on these matters. My impression was that we might cultivate this bacterial growth by changing the chemcal conditions, as much as by changing conditions of temperature—possibly more so. The experiments that we have conducted show that with alkaline conditions the bacterial growth in our slip was greatly increased; with acid conditions, bacterial growth was destroyed.

Another theory of mine is that the plasticity does not so much depend upon the acid condition, as upon a gelatinous secretion made by the bacteria in the slip. It seems to me that a wide study of this would be abundantly interesting. I think the most favorable conditions can be controlled, not only by atmospheric and heat conditions, but also by chemical conditions of acidity or alkalinity; and that by regulating these conditions, we can handle this fermentation much as we please. I think you will all be interested in looking at these microscopic exhibits. They are certainly open to you, and I think will come cheaper to you than they did to me, perhaps.

Mr. Stover: Just a word as to what Mr. Burt said as to acid conditions. Immediately on finding sulphuretted hydrogen, the question arose as to where we got the sulphur. Then came the question, couldn't we get the same results by adding a dilute solution of sulphuric acid. We took some fresh white mixture that had not been inoculated, sterilized it first, so as to be sure there would be nothing alive in it to start with, and added a diluted solution of sulphuric acid with the hopes of doing the same thing quicker. I thought if we could get the result this way in half an hour we could abandon the clay cellar. But I found it would not do it. But a small amount of the dark blue slip-mixture placed in some of this same sterilized material gave the full benefits of ageing in a very short space of time.

THE EQUIPMENT OF A MODERN PAVING BRICK PLANT.

BY

WILLARD D. RICHARDSON, CLEVELAND, O.

While the American Ceramic Society must be concerned chiefly with the higher branches of clay working in which there is a greater scope for science and art, yet, we who are permitted to enjoy membership, even though engaged in the more common and less skillful department of brick-making, expect some light to be shed upon the obscure and uncertain places in our path, some investigations to be made by the society, some facts to be brought out at these meetings that will result in more exact and definite knowledge of the processes we employ.

That noble man, that eminent ceramic technologist, Dr. Herman H. Seger, did not confine his investigations to the problems of the potter, but gave much of his time to the study of the processes of the brickmaker; and the results of his labors, which our society is about to make accessible to the clayworkers of America, are as valuable a contribution to us who are making the common and homely brick as to you who are making the beautiful and artistic pottery wares.

So we brickmakers are greatly benefitted by our connection with this society, though we can give little in return for what we receive. We pay our dues and attend these meetings and listen to the learned discussions of your scientific clayworkers, and absorb and assimilate what we can, and are stimulated to better, more progressive work and perhaps thereby be enabled to enlarge our sphere of action.

For my part, I would not presume to take up the time of this meeting with what I have to offer, but our secretary

has assumed the responsibility of placing me upon the program, and I am pleased to have the honor of presenting a set of plans that I have made for the construction of a plant for the manufacture of sewer and paving brick. The drawings have been made in such detail by architectural draughtsmen that very little explanation is necessary. My experience has taught me that before starting to build, it pays to have every detail carefully drawn out on paper. However well one may have thought out or sketched out his plans, when he comes to assemble the complete details, he will run up against some conflicting arrangements and be obliged to make many changes, as one change generally leads to another. It is much cheaper to make these changes beforehand, on paper, than in wood and iron and brick after the building has been begun.

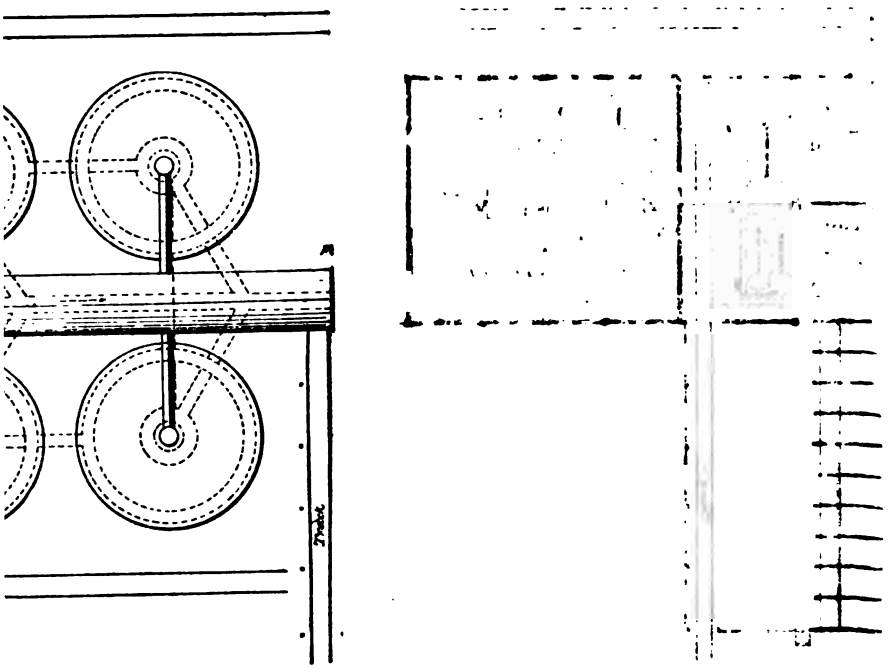
In planning a brick plant, due consideration must be given to the special purpose of the proposed establishment, and to the special local conditions, such as the size and topography of the site for the building, the location of the clay and the railroad, etc. These local conditions must always determine to a greater or less degree, the general plan to be adopted.

The plans herewith presented are for a paving brick plant, which it is expected will be built in the spring, about one mile south of this city. The shale to be used is situated above the level of the factory, to which it will be conveyed by a gravity railway. The direction from which the shale must come is directly opposite to that from which must come the spur from railroad for receiving coal and shipping brick. These facts necessarily influenced me in settling upon the general arrangement of yard.

I will describe briefly the equipment of the plant shown on the drawings:

The cars of shale enter the factory at an elevation of twenty-two feet and are dumped into hoppers built so that one man, by operating gates from below, can attend to the feeding of three or four drypans, — or the shale can be dumped outside of the hoppers in the storage room and fed to pans, in stormy weather, in the usual manner.

These pans are placed in the pulverizing room back of



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ODERN PAVING BRICK PLANT.

ility of placing me upon the pro-

These pans are placed in the pulverizing room back of the press room in a row on a line parallel with the general progressive movement of the clay and brick, so that the clay is easily conveyed from the screens to a central hopper for feeding to pug mills.

The simple stationary inclined screens are used, the clay being elevated from pans to sufficient height to permit of these screens being twenty-four feet long. The width of each screen is six feet, and in order to secure a uniform distribution over the surface of so wide a screen, the clay is dumped from elevators into an U trough conveyor having an adjustable slot in bottom, over the screens. The tailings from the screens pass to the shale hoppers instead of directly to the pans. The screened clay is collected at the foot of the screens by a conveyor that carries it to the hopper above the press room.

The hopper for fine clays is in an upper room separated from the press room by dust-tight partition and floor, the lower part of the hopper projecting through this floor into the press room, and above and between the pug mills. This hopper is also an automatic feeder, supplying, when in operation, a constant uniform quantity of clay to the pug mills, making uniform tempering an easy matter. The capacity of this hopper and feeder is sufficient for the two largest pug mills shown on drawing. These pug mills are placed back to back, at right angles to brick machines, for convenience in receiving and discharging clay.

The brick machines are the standard auger type, largest size, with automatic cutting tables and separating belts.

In this special factory, one machine is to be used for making sewer and building brick, not repressed, and the other for the large size paving block demanded in this market. With the latter machine is shown one double die repress with capacity probably sufficient for this factory, when making these large blocks.

The cars, loaded from the separating belt or repress, pass to a transfer track, just back of the machines at the rear of the press room, and thence to the receiving tracks of the dryer.

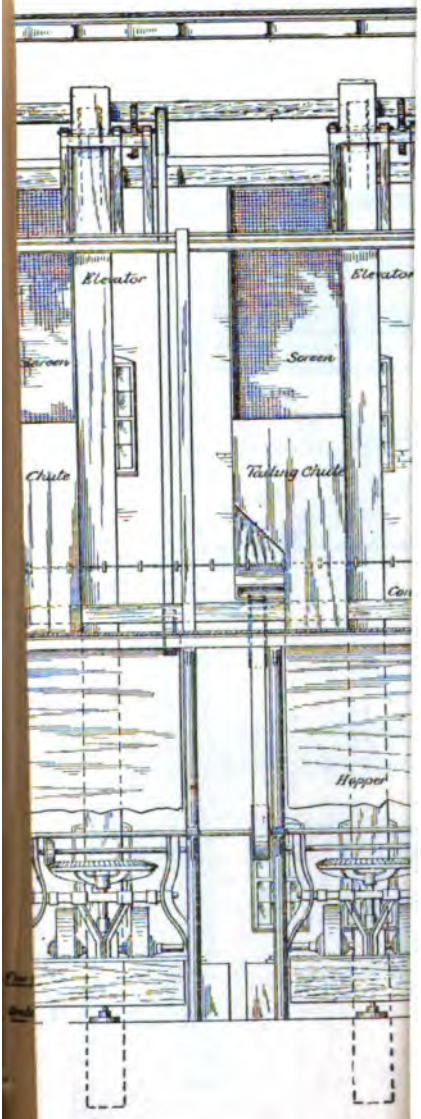
The power is transmitted to machinery by a line shaft extending through the center of the building and supported upon cast iron columns with brackets cast upon them.

The boiler room is situated at the lower end of dryer, at the head of the kilns, for convenience in receiving fuel and in furnishing steam for blower engine, and so as to utilize the combustion gases for dryer, if desired, and also exhaust steam. The boilers, of the water-tube type, have induced artificial draft, being connected so as to be operated by separate fan discharging into atmosphere, or by the dryer fan discharging into dryer.

The engine room, between the boiler room and the press room, contains a 250 H. P. Corliss engine for running the factory, and an automatic engine for running generator, dryer fans and machine tools. The machine tools situated in engine room are lathe. planer and drill press. The generator is for running motors that operate fans for induced draft of kilns and also for lighting.

The buildings, you will notice, are practically fire-proof, the walls of brick and the roof framing of steel. This roof frame is covered with matched pine one and three-fourth inches thick and upon this, slate or roofing tile. The underside of the wood, between the rafters, is to be covered with two coats of lime whitewash, or other fire resisting paint. The floors are mostly of concrete and nearly all posts and beams of iron.

The dryer, constructed for the most part in the usual manner, consists of a series of tunnels separated by brick walls — each tunnel containing two tracks 100 feet long. The receiving tracks are twenty-three feet long and the cooling tracks forty-four feet long. The construction of the dryer is absolutely fire-proof, the roof, of course, being the special feature. Upon the top of the division walls, which are six feet and nine inches between centers, is constructed four inches of concrete, made from vulcanite cement, gravel and coal cinders, reinforced with a net work of iron strips. Upon this concrete is a course of hollow book tile laid in cement, and upon the tile is a gravel roof, the felt being stuck to the tile. This roof slopes from the receiving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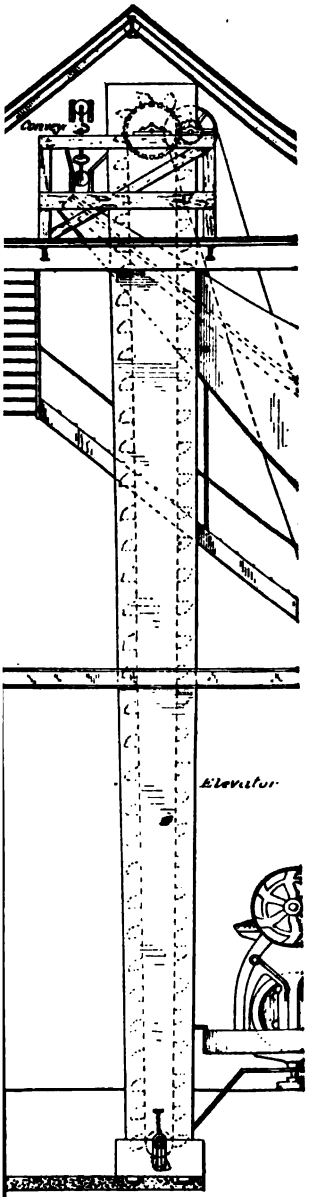
Elevator

end of the dryer to within ten feet of the discharge end,—one-fourth inch to the foot, this being the slope of the track in the tunnels. The last ten feet of roof at the discharging end is built up to slope backwards, forming a valley. In this valley are openings into 4-inch conductor pipes laid in each wall, and passing downward to the sewer pipe drain below.

The special feature in addition to the roof construction is the slope of one-fourth inch to the foot in the tracks from the receiving end, the last ten feet being level, and the method of moving the cars through the tunnels. The details of this latter are not shown on these plans, but can be briefly described.

The cars are coupled together automatically when placed in the dryer, so as to insure no break in their ranks as they move by gravity down the decline. At the bottom of the decline is a device for braking and stopping the cars. By throwing a lever just outside the entrance of the tunnel, the forward car on any track is disengaged and the car back of it is blocked. After the forward car is removed, the lever is reversed and the other cars let down and blocked, each car being uncoupled automatically about ten feet from the end of the dryer. Thus one car is always free to be removed from dryer, and a car can be put into dryer at receiving end without the necessity of pushing other cars.

The heat for drying is obtained from cooling kilns by means of a fan — the distribution of heat being in the usual manner. The special feature, however, is that the saturated air is removed from the dryer through an underground flue by means of a fan coupled with the fan that furnishes the hot air, both fans being upon the same shaft and driven by the same belt, the driving pulley being in the boiler room upon the shaft extending from the engine room.

In making plans for a brick plant, especially for the manufacture of paving brick, one's chief study must be in designing the kilns for burning. In no other department of brick making, is there greater need of improvement, notwithstanding the large number of patented kilns on the market. Many of these patent kilns possess positive, dis-

tinct points of merit and are well adapted for special situations, but the trouble is that each advertiser recommends his kiln for all purposes and situations. However, I could find no kiln advertised that would meet my requirements for a modern paving brick plant.

The kiln for my purpose must have the following features:

1. Down draft.
2. Uniform distribution of heat.
3. Economical in operation, saving fuel, labor and time, and so constructed as not only to utilize waste heat, from cooling kilns for transmission to dryer, or for heating air for combustion in kiln furnaces, but also it must utilize the waste heat escaping from the hot burning kilns in heating up other kilns. In other words, it must have the advantages of the continuous kiln, without its disadvantages.
4. Durability — costing little for repairs.

The first question to be determined was whether to have separate kilns, each of which could be burned independently, if desired, or to have the several ovens or chambers so placed in juxtaposition as to preclude one being burned independently of the others. I was influenced somewhat in this by the judgment of friends who have had large experience with continuous kilns, and also by the fact that in building separate kilns, I would be following along the line of my previous practice, and would feel more confident of such results as I wish to obtain. I do believe, however, that a continuous kiln can be built that will be highly satisfactory for paving brick.

Having decided upon separate kilns, the next point to determine was the form of the kiln — whether round or rectangular. My experience with both forms enabled me to decide at once in favor of the round kiln, chiefly for two reasons:

1. Because the round kiln is less expensive in first cost and especially in repairs.
2. Better distribution of heat and fewer underburned brick.

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The stack question came up next for consideration — whether one or more stacks, and where situated. You will notice that I decided upon the single stack in center of kiln. The advantage of this arrangement are:

1. High temperature of stack, hence a low stack.

2. Heating up the stack from the first firing means a strong draft in the early part of burning, giving freedom from whitewash and shortening of time of burning.

3. A kiln of larger diameter with hard brick to the center.

The disadvantage of stack in center of kiln are:

1. Inconvenience in setting.

2. Space occupied in kiln.

These disadvantages I have found not so great as I first thought. The stack in the center of a large kiln is not much in the way — in fact, hardly more in the way than a stack in any other place. The space occupied in the kiln is for a thirty-foot kiln only about two per cent. of the inside kiln space and for a thirty-six-foot kiln, about one and one-half per cent.

The special feature of the kilns for this plant is the system of utilizing the hot gases escaping from the burning kilns for heating up other kilns, and likewise of using the cooling kilns for heating the air for combustion. To accomplish this the kilns are connected by an underground flue, extending from the center of each kiln to a circular distributing flue in the next kiln ahead. This circular flue is in the hub of the kiln, with openings into each fire box, adjustable by dampers. The movement of the gases is induced by a fan connected to the stack of any kiln by a cylindrical steel flue, as shown in drawing. The fan is portable, being erected upon a car moved along an elevated track between two rows of kilns and operated by an electric motor directly connected.

I calculate in this manner to be able to draw the heat from a hot kiln through at least two other kilns and that by the time that the kiln under fire is finished, the kiln just ahead will be partially or wholly incandescent, so that heavy fires can be put into the furnace at once. There will thus

not only be a saving in fuel, but a considerable saving also in the time of burning.

Whether I accomplish all that I expect by this system or not, I am confident that I can accomplish enough to more than pay for the additional expense of construction. However, each kiln can be burned independently by natural draft, if desired.

Now, gentlemen of the American Ceramic Society, I have given you the result of my thought and experience upon the subject assigned me. I have no interest in influencing anyone to adopt any of the suggestions I have made. I have tried to do my duty in presenting this paper, and I hope to have the benefit of your criticisms.

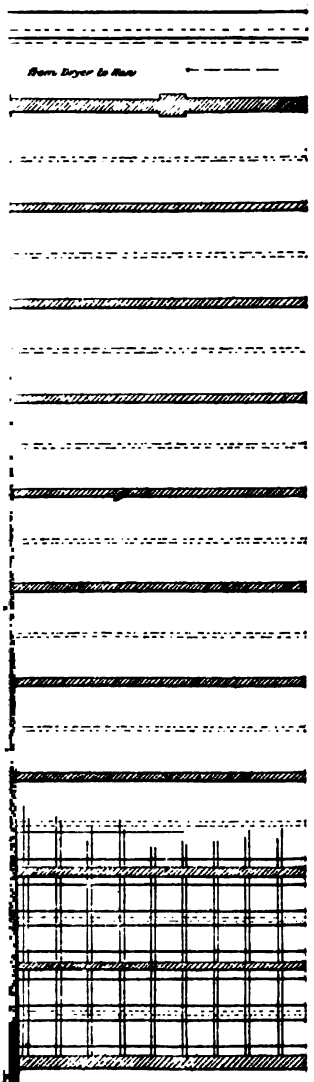
DISCUSSION.

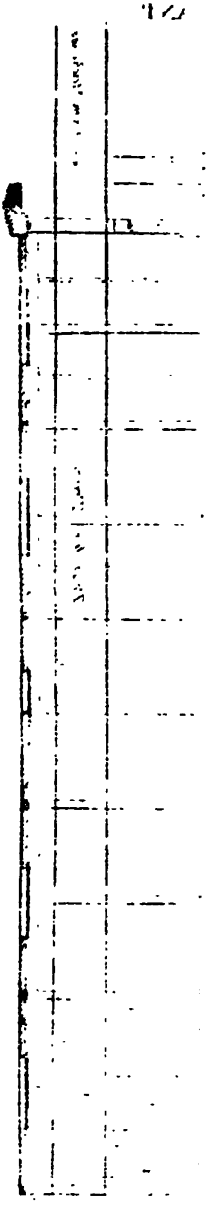
Mr. Lemon Parker: How much grate surface do you provide as compared with the size of the kiln?

Mr. Richardson: I have not gone into many of the details, because I did not know whether it would be of sufficient interest to those who would listen to it. There is really no grate surface at all. These are thirty-six foot kilns, with twelve furnaces to each kiln. The fire-hole is two feet wide, without grate bars. As shown in the plans, the firing door is covered in the usual manner with a fire clay slab, reinforced with iron bands. The coal is thrown upon a coking table, made partly of cast iron and partly of fire-clay tile and shoved down upon the fire just before a fresh supply of coal is added. A furnace, similar to this, is used quite a good deal in Ohio, but this drawing here is not exactly, perhaps, as I would make it for this kiln. Since this drawing was made, I have built furnaces which I think are better than these. But they are built on the same principle—down-draft furnaces, with a coking table and no grate bars.

Mr. Parker: We use fourteen fire boxes for a twenty-five foot kiln.

Mr. Richardson: Many people will not use as many fire boxes as I have shown here. I never built a kiln larger than thirty feet; on that, I used ten fire boxes, and had no trouble in getting plenty of heat in the kiln.


From Dryer to Hose



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Mr. Parker: What cone did you burn to?

Mr. Richardson: In the kilns I referred to, we burned iron mottled, fire-flashed brick, cone eight down, on the bottom.

Mr. Parker: We burn cone ten.

Mr. Richardson: That is a higher temperature than is usually required in the brick business. Still there would be no difficulty in getting that temperature. But probably if you require that temperature, it would be well to have wider furnaces, or more of them.

Mr. J. Parker B. Fiske: I have been exceedingly interested in this paper and in the drawings, not only on account of their technical value, but because it seems to me this paper marks an era in the brick manufacturing world. We have had presented here a carefully prepared set of plans for a brick plant, based on an experience of many years in brick manufacture, and worked out by a competent engineer. The mechanical engineer up to the present time has not applied himself very largely to the designing of complete clay-working establishments, particularly those for manufacturing brick.

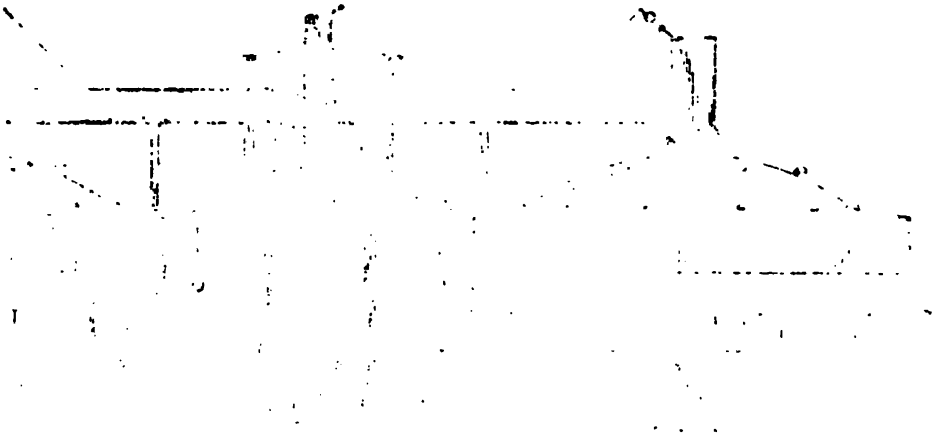
The manufacturer, as a rule, has had to gather such points as he could from his neighbors, coupling with these his own experience, and then with a very meagre set of plans (if any at all), has had to commence the construction of his plant. And I venture to say that the average brick plant, which, like Topsy, just grows, without any predetermined plan, is hardly ever well commenced before the builder finds that some things are wrong, some parts don't work together to advantage, and he wishes he had built differently.

Now, the building of any large manufacturing establishment today, outside of brick-making, is rarely ever undertaken without the most careful consideration of every feature entering into the construction and operation of the plant. A modern steel or cotton plant is designed by the most competent engineers before anything else is done. I believe that the losses which the brick manufacturer suffers today, more or less in every plant, are largely due to the

fact that the whole arrangement is not carefully considered on paper, before building is attempted. Now, of course, no matter how carefully a plant is worked out on paper, there will be some things discovered during the process of construction on which should be altered; but with a careful consideration in advance of every feature of the complete plant, as in this case, from the arrival of the raw clay to the finished product, there cannot fail to result economy of labor, economy of fuel, and cost of maintenance far ahead of that in a plant otherwise erected.

There are many points about this plan in which I am interested, and which I would like to discuss with Mr. Richardson, but I will not take the time of the society for that. I would like to ask one or two questions, however: One is, what particular advantage Mr. Richardson sees in the arrangement of isolated kilns, coupled together for continuous operation, over the construction of a kiln where the chambers are all built in one structure and operated continuously?

Mr. Richardson: Well, as I stated in my paper, I do believe that a continuous kiln can be specially designed to burn successfully any kind of product. But, in making plans for a plant in which you are to put your own capital and that of your friends, you feel that you dare not be too radical for fear that something might be wrong somewhere. I have, in fact, gone all over that question. I have been first on one side and then on the other, and I have tried to include the advantages of both systems as far as possible. The advantage of this plan is that any kiln can be burned separately, independent of the rest of the plant whenever it is desired to reduce the output or to make repairs; and also, that for paving bricks, probably better results can be obtained. The vitrification and proper annealing of paving brick no doubt can be done in a continuous kiln, but there were too many questions in my case as to just how it was best to do it, and whether it would be a decided success. I know the continuous kiln is being used for paving brick in a few places, but to the best of my information, not



as successfully as the down-draft kiln. It may be owing to the fact that the continuous kiln is not properly designed for this special purpose. I am a little inclined, myself, to go wholly into the continuous kiln line, but have been afraid to take the risk in such a plant as I have here designed and where all depends on the efficiency of the single kiln system.

LEAD FRITTS AND THEIR ADAPTATION TO POTTERY GLAZES.

BY

WILTON P. RIX, NEW CASTLE, ENGLAND.

The attention of the English potters has of late been directed to the relative value of lead in a raw and fritted state, by the proposal to enforce regulations forbidding the use of raw lead in the manufacture.

Statistics support the statement made that the use of fritted lead reduces the noxious quality of the glaze, but it has been shown, beyond doubt, that in order to obtain complete freedom from plumbism, it is necessary that the lead contained in the fritts used should be insoluble in dilute hydro-chloric acids; that is, that the lead should be present only in the form of bisilicate or compound silicates complying with the above tests.

This matter has really assumed still greater importance owing to the comparative failure of the attempts made by a number of the English earthenware manufactures to obtain satisfactory leadless glazes, capable of being worked under the ordinary conditions. The average loss has been found considerably greater with the use of such glazes than in the case of those containing lead, owing to the greater care required in the manipulation and firing such glazes.

In attempting the substitution of fritted lead glazes for those compounded with raw lead, various important considerations arise with regard to their adoption. These relate to apparent disadvantages which has been raised against them; also to the possibility of adjusting the fritts to the present requirements of the various branches of the trade.

The objections may be summed up as follows:

First. The additional cost involved in fritting as regards fuel, wear and tear and labor.

Second. The loss of lead by evaporation and absorption in kiln fritting.

Third. The difficulty of producing insoluble fritts of sufficient fluidity to allow of their introduction into the softer glazes, without a sacrifice of the brilliancy and soundness of the glaze.

Fourth. The increase of the difficulty of manipulating and firing such glazes.

With the adjustments of such fritts to the present glazes and conditions, the question at issue may be stated as follows :

The continental potters appear to have realized that the use of fritted lead assists the immunity of the lead worker from plumbism; indeed many of them have adopted fritting with promising results, while a few of the most important works have produced glazes from fritts of very low solubility.

The temperatures at which such continental earthenware glazes are matured vary from 06 to 01. The temperature for maturing English earthenware glazes varies from code 05 to cone 03. The percentage of lead used in English earthenware glazes varies from 18 per cent. to 25 per cent. The percentage of lead used in English china glazes varies from 3 per cent. to 20 per cent. The percentage of lead used in English majolica glazes is from 35 per cent. to 55 per cent., or even 60 per cent. The temperature for maturing majolica glazes varies from cone 010 to cone 05.

These being the existing conditions the questions arising are:

First. Does the substitution of lead fritts as in raw lead involves any alteration in actual properties of the glaze components in order to obtain results similar to those of raw lead glazes?

Second. Does the use of fritted lead involve any increase of firing temperature, or any different method of carrying on the firing process?

Third. Is the fluidity of the glaze affected by the character of the lead fritts used; that is whether bisilicate or double silicate or compound silicate is used?

Fourth. Can glazes of sufficiently high fusibility be obtained for the manufacture of plumbiferous majolica

glazes, fired at Seger cone 010, when these are compounded of lead bisilicates or double silicates, the maximum solubility of the glaze not exceeding, say 3 per cent.?

Fifth. How far is it possible to introduce other fluxes into the fritt mixtures, thus increasing fusibility and yet still maintaining the requisite insolubility of the lead present: and having also due regard at the same time to the effect in use of such fritts?

(*a*) On the working properties of the glaze in firing.

(*b*) On the tints of colored majolica glazes.

(*c*) On the fluidity of the glazes when fired at the customary temperatures.

On many of these points the English manufacturers appear at present to be at variance. Some imagine that the use of lead fritts cannot be carried out: while others show satisfactory results and have no objection to raise as to their adoption. Probably this discrepancy arises from varying conditions coupled with a want of efficient manipulation, and also insufficient attention to details in the case of some who have failed.

It is obvious as regards the use of lead fritts in the glaze that the lower the firing temperature, the greater the amount of lead required. When this amount exceeds that present in a bisilicate fritt, new difficulities arise in the production of a double or compound silicate, capable of holding in combination such fluxes other than lead as will affect the necessary fusibility of the fritts and consequently of the glazes.

It will be seen that various other existing questions are involved which will readily suggest themselves to members of the society, and on which at present little accurate information exists, but those mentioned above are more than sufficient to occupy the scope of the present paper. I therefore propose to limit the discussion to the points first named.

(1) *The possibility of substituting lead in fritted form for raw lead without change of proportions used.*

On this point my experience does not bear out the statement made by some manufacturers, that more lead is

required to produce the same results if introduced in the form of fritt. I have made comparison of the same glazes worked under both methods side by side in the same saggers, and have repeated these in different ovens. The results do not vary in any substantial respect provided that the total contents of the glaze remains the same, and the calculations of the new formulæ are accurately carried out—the raw and fritted glazes for the lead may be substituted for raw lead.

(2) *Does the use of fritted lead involve any increase of firing temperature?*

Provided the total contents of the composition of the raw and fritted glazes are the same, the raw and fritted glazes will be found to fuse at the same temperature. There is, however, some indication that some lead fritts take longer to mature in firing than others. The point of incipient fusion must also be placed somewhat higher as regards temperature. There is evidence of this fact in composition of fritted glazes now in use on the continent by some important manufacturers, who add a very small percentage of raw lead to the glazes, obviously for the purpose of promoting the incipient fusion at a lower temperature. I attribute to these questions of incipient fusion and length of firing, the difference of results obtained by various potters. It is probable that the firing is carried on by different methods, or that the uniformity of heat in various positions in the oven is less with some than with others.

(3) *Is the fluidity of a fritted lead glaze affected by the character of the lead fritt used; that is whether bisilicate, double silicate or compound silicate.*

On this point there appears to be strong evidence that the fluidity is thus affected: that is, apart from the question of fusibility the glazes when fused are more or less viscous, according to the composition of the fritt. It is, however, open to further research to demonstrate how far this fluidity of the glaze at a good temperature if affected by the heat at which incipient fusion takes place, and also by the length of time required to mature the glaze in firing, without increasing the temperature. This issue is one of considerable importance to the potter in adopting fritted glazes for use on

goods printed under glaze. My own experience over a limited area is that double silicates of lead show less fluidity than bisilicates if worked under the same conditions, the equivalents being of course duly regarded in the fritt composition.

(4) *Can glaze of sufficiently high fusibility be obtained for majolica colors, firing at cone* 010 *by the use of bisilicates or double silicates of lead, still maintaining a solubility not exceeding, say three per cent.*

Before discussing this question it is necessary to define the amount of fluidity required in the glaze. For plain tiles, needing only uniformity of color and texture, the property of extreme fluidity and transparency is often less important than in the case of finely modelled relief tiles and blended glazes, in which the artistic effect is mainly due to the falling of the glaze into the deep hollows, and the ready mingling of the colors with each other during the firing.

In the latter case considerable difficulties arise in producing suitable glazes with bisilicate lead fritts. Many glazes now in use for this purpose contain as much as fifty to sixty per cent of lead oxide, and it is obvious that even supposing the bisilicate fritt by careful preparation is obtained under two per cent. solubility, the high percentage of lead contents required in such glazes leaves no room whatever for the introduction of other glaze materials to improve its working properties.

It must also be pointed out that the method adopted of obtaining some colors by the dilution of a boro-silicate colored lead fritt with transparent lead glaze, is no longer available when the two per cent. solubility is exceeded, and even if the raw lead is replaced by lead fritt. The high solubility of such boro-silicates forbids their use in these conditions, such as the difficulty of obtaining low solubility with sufficiently high lead contents in the glaze to insure the extreme fluidity on which the beauty and richness of many soft majolica glazes depend. In this branch it appears to me there is ample scope for further research, and it is probable that eventually the introduction of a new pallette of

colors may be involved — or a new type of decoration less dependent upon the property extreme fluidity.

In discussing the above questions it must be understood that the production of glazes having a maximum solubility of two per cent. has been kept in view, and also the maintenance of existing conditions in English pottery manufacture as to composition of glazes and firing temperatures, and adaptation to existing bodies.

The difficulties are obviously much diminished as the firing temperature is increased, because the lead contents of the glazes can then be proportionately dimished.

DISCUSSION.

Professor Charles F. Binns: This lead question has been an interesting study, and has cost English manufacturers a good deal of heart-burning and anxiety. One important point has been brought forward in the relative solubility of lead fritts. It is found that lead fritts are more soluble with boracic acid than without. This was proved out by a Belgian manufacturer. I doubt if this fact will affect the bulk of small manufacturers in England, for they are troubled enough in making one fritt, without making two. It is scarcely known here how far the practice exists among the smaller English manufacturers of buying ready-prepared glazes. Quite a number buy it ready prepared and have no more idea what is in it than the man in the moon. This operates against anything like a change in glaze. As the matter stands now, the English manufacturers have been given eighteen months in which to make experiments, and my view is that the burden has been materially lightened as to the potter.

I want to say a word about Mr. Rix's last remark regarding the expansion of the ceramic society. The North Staffordshire Ceramic Society has been established since we have been in existence. How far they have thrown their net around the manufacturer, I do not know. They meet once a month for the discussion of minor problems; and I have wished sometimes that we could meet once a month and have one thing at a time. We are in great danger of ceramic dyspepsia from the full meal we have had here.

One more point. I wish Mr. Rix had defined his bisilicate and double silicate and compound silicate. We ought to have further information on this point.

Mr. E. E. Gorton: My experience differs somewhat from Mr. Rix's on one point, in regard to the fritting of the lead. I have found that where lead is fritted, and the glaze is not a boracic-acid glaze, it does require a higher equivalent of lead to get the same results. I presume it is due to the volatilization of lead in fritting.

Professor Binns: I ought to make another remark. This question of the solubility of the fritt created quite a good deal of disturbance. The English regulations required the inspector to take samples from the dipping tub, and if he found more than two per cent. of raw lead the manufacturer could be brought to account. The point has arisen as to what constitutes solubility. Mr. Jackson carried out a line of experiments and found that solubility was directly commensurate with the size of grain. By repeated grindings, he found he could dissolve more and more as the fritt became finer, and it seemed that if he could go on long enough he could dissolve the whole. His conclusion was that a coating of insoluble lead was formed on the grains of fritt, and that the grinding removed this each time.

Professor Orton: Wouldn't they be driven to construe the question as to the amount of lead in solution in the dipping tub?

Professor Binns: Yes — not in solution in the dipping tub, but in suspension. The per cent. of raw lead in the dipping tub is one thing and the solubility of lead fritt is another. The government said, "You must not use more than a certain per cent. of raw lead." The manufacturer laid, "We can't make glazes with that amount." Then the question arose, could not the lead be rendered insoluble. It was suggested that by fritting it might be rendered insoluble. They can use raw lead in the dipping tub, providing it is not above a certain amount.

Professor Orton: Does English legislation forbid the use of a glaze in which more than two per cent. of lead is soluble?

Professor Binns: That is what was proposed. We were obliged to provide overalls, caps, nail brushes, soap, towels, etc., in order that the men and women should be compelled to wash themselves, and clean their nails. The whole trade is in revolt owing to this ill-judged and "grand-mother" legislation, from which we in the United States glory that we are free. The next point which arose was in reference to a series of rules embodying the limits of raw lead which could be used. The investigation which was made has entirely broken down on the government side, and it has nothing to say at present, though it may have eighteen months hence. It has been demonstrated that if the regulations are enforced the whole pottery industry of England will be killed.

Mr. Burt: I always understood that the poisoning, as a rule, was due to the inhaling of lead dust. I don't see any direct connection between lead dust and the question of solubility, whether two or three per cent. is in solution.

Professor Binns: The issue is somewhat simple. It is due to the effect of the juices of the body, particularly the gastric juice upon the lead oxide; and the effect of dilute hydrochloric acid is considered to be the same as that of the gastric juice. The point was whether lead oxide, if soluble in hydrochloric acid, might not be absorbed by the skin. Not only might this be so, but sometimes the work-girls would carry candy in their pockets and feed themselves while their hands were covered with lead. It gets into their hair, and the body pores can take in a vast amount of the lead salts. The effects are shown to vary with different persons. I have known a case where a girl was put to work trimming a porcelain glaze not containing more than four per cent, of lead oxide, and she sickened in a week. And there are cases where women have worked twenty years and not felt the slightest ill effect. It is largely a matter of tempermanent, and we always made it a rule when anyone showed any signs of sickness to put them elsewhere, only keeping in that department those who showed themselves to be immune.

THE STRUCTURE OF SILICATE MIXTURES.

BY

HARRISON E. ASHLEY, S. B., NEW BEDFORD, MASS.

"Based upon the microscopic examination of thin sections of minerals and rocks, observations were recorded in 1858 by Dr. H. C. Sorby, member of the Iron and Steel Institute, in a paper on the microscopic structure of crystals, indicating the origin of minerals and rocks (Quarterly Journal of the Geological Society, vol. XIV, page 453), and in October, 1867, by the late Mr. David Forbes, member of council and foreign secretary of the Iron and Steel Institute. These observations gave birth to the special science of petrography." *

"From the introduction of microscopic methods of investigation up to a date about eight or ten years ago, the energies of practically all students of the subject were devoted to observing and recording mineralogical and textural differences, and the subject of chemical composition received but slight attention. It was revived, however, toward the close of the eighties by W. C. Brögger, then in Stockholm, and in the course of time has received wide recognition and employment from many others.

Petrographers are now accustomed to recast an ordinary chemical analysis by dividing the several percentages by the molecular weights of the corresponding molecules, so as to obtain a series of numbers, which are called the 'molecular proportions' or 'molecular ratios.' These quantities indicate the relative numbers of the several molecules in the rock magma, and in that respect are more significant than are the percentages. . . . . The molecular composition of the common rock-making minerals are now quite accurately determined and understood, and using them

* The Metallographist, (1902), 5 [2], 146.

it is often possible to calculate from the molecular proportions furnished by a rock analysis, the percentages of the several minerals in the rock. The calculations are usually checked in a general way by a study of thin sections." *

In a recent joint report by W. H. Weed and L. V. Pirsson, † the latter presents recalculated analyses of a large number of igneous rocks. He first ascertained by microscopic examination what minerals were present; then from chemical analysis and molecular proportions what percentage of the several minerals were present. Prof. J. F. Kemp, the author of the article on 'The Re-calculation of the Chemical Analysis of Rocks,' from which the two preceding paragraphs were copied, shows in detail in that article the method of recalculation.

The present writer, in an article § on slag-constitution, has shown that the freezing or melting points, or less exactly, the formation temperature, or the total heats of solidfication will show the constitution of such mixtures at the temperature of fusion.

Chemical methods of separation, taken by themselves, have not been very successful in showing the structural constituents of silicate mixtures.

It is most satisfactory to rely, not on a single method of investigation, but on the joint use of several methods. It is desirable to supplement chemical analysis and fusibility determinations by microscopic examination. The latter often presents special difficulties. The slag or ware may be unsuited for the preparation of thin sections, or if suited, few persons have the necessary equipment for their preparation, or considerations of time and expense may be prohibitory. Silicate mixtures are seldom examined on the surface of thick pieces by reflected light, as it is difficult to find any apparent differences of structure. With metals and alloys on the other hand, surface examination by reflected light is

* School of Mines Quarterly, (1900), 22, [1], 76.

† Geology of the Little Belt Mountains, Montana, by W. H. Weed, with a report on the Petrography by L. V. Pirsson, XX. Ann. Rep. Dir. U. S. Geol. Survey, III., 257.

§ Slag-Constitution, studied by means of the Tri-Axial Diagram, with Rectangular Co-ordinates.—Trans. Am. Inst. Min. Engineers. Nov. 1901.

the universal method. Differences in structure are shown by the unequal action of polishing agents, of etching agents, and of oxidation, or other coloring effects. With silicate mixtures, so far as the writer knows, nothing has ever been accomplished by differences in wear or by development of colors, and but little by etching with fluorides or alkalies.

A phenomenon met in laboratories may offer a means of progress. The writer has frequently noticed that the first portion of vapor condensing on a cold glass surface, for example, the underside of a watch-glass upon a beaker whose contents are being heated, is not uniformly distributed over the whole surface, but is in small patches. Each of these patches seems to be uniformly covered, and differs in density from the patches touching all around it. The whole effect resembles the microstructure of a slowly cooled very crystalline metal. Each patch of moisture may indicate the extent of the external surface of one crystal* of the glass. With further condensation the differences are obliterated.

It seems likely that the first microscopic deposit may be more highly differentiated than that that appears to the naked eye, more destinctive in character, and more certain of positive appearance. While water vapor is a fugitive medium, there are multitudes of definite chemical substances capable of being sublimed with the formation of fairly stable deposits.

It is possible that the selective action of the silicate mixture crystals may even go so far as to attract or retain, substances having the same crystal system and molecular volume, while rejecting substances of unlike properties. This may open a way for the study of molecular weights of solid substances.

The writer regrets that he is not at present in a position to test and develop the method of research here proposed.

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*H. M. Howe: "What is the essence of crystalhood?" Metallographist, (1902), 5, [1], 52.

THE RELATION BETWEEN THE CONSTITUTION OF A CLAY, AND ITS ABILITY TO TAKE A GOOD SALT GLAZE.

BY

LAWRENCE E. BARRINGER, E. M., SCHENECTADY, N. Y.

The subject of salt-glazing has received but little attention from investigators in ceramic work since its introduction by Flemish potters in the thirteenth century. Considering the extent to which the process is employed in the manufacture of clay products in this and other countries, and the fact that but little is known of the reactions of the process, and still less as to what materials are most suitable for such treatment, it is a matter of surprise that no attempts have been made to throw some light on this important branch of clay glazing.

In English ceramic literature the subject has received little or no consideration. The German writers have given some attention to the subject, but the bulk of the articles are either historical or deal with the actual practice in accomplishing the desired results.

The only article of real investigation which the writer has been able to find, is that on the action of iron in clay during salt-glazing, by Jos. Knett, published in Thonindustrie-Zeitung, 1896. Knett's experiments, while leading to highly interesting results, do not attack the truly fundamental principles of the process.

A desire to at least awaken a little interest, if possible, in original research along the line of salt-glazing, led the writer to make the investigations noted in this paper.

It is a well-known fact that some clays salt-glaze better than others, while some are not at all able to be glazed in this manner. Furthermore, the character of the glaze is different on different clays, as regards color, hardness and finish.

In order to draw a line if possible between salt-glazing and non-salt-glazing clays, it was decided to attack the subject by analysing both the clay and glaze from a factory where the work is successfully carried on, and from this basis vary the composition of the clay until its limit of ability to take a good glaze was reached. To this end, the clay used at Logan, Ohio, by the Hocking Clay Manufacturing Company, was selected. The salt-glazing done by this concern is of a high order, and their products are first-class representatives of salt-glazed clay wares. The process is here applied to bricks, sidewalk tiles, sewer pipe, garden urns, etc. The clay used is a fire clay of the Lower Kittanning horizon, the vein being pretty well weathered.

The methods used in securing samples of the clay and glaze for analysis were as follows:

SAMPLING THE BODY AND GLAZE.

In order to obtain a fair average sample of the clay as it entered into the product, a barrel was placed near the chute leading from the bin containing the screened clay from the dry pan, into the wet pan. Into this barrel the wet pan attendant threw a little of the ground screened clay at intervals, filling the barrel at the rate of about one-fifth each day. In this manner a fair average sample of the clay used in a five day's run was obtained. The contents of the barrel were then dumped onto a clean cement floor and thoroughly mixed, finally quartering down systematically until about thirty pounds remained. This was placed in an iron ball mill with flint pebbles, and ground fine. The finely ground clay was then spread on a table and mixed thoroughly, and then quartered down until a sample of a few hundred grams was obtained, which was bottled and labelled, and constituted the sample from which the analyses were made.

The glaze sample was obtained by chipping the salt-glaze from bricks taken at random from different stock piles, the only consideration being that the glaze should be sufficiently thick to chip off nicely. Bricks on which the glaze had run more or less towards one edge were chosen, and the glaze was carefully chipped off along this edge with a small cold

chisel and light hammer. About twenty grams of the glaze was secured in this way. From this, the sample for analysis was secured by selecting pieces of clear glass from the latter, rejecting any chips which showed any indication of adhering body. The chips of glaze thus selected were ground fine in an agate mortar, and weighed out for analysis.

ANALYSIS OF THE BODY AND GLAZE.

Two carefully made analyses of the clay gave the following results:

	No. 1	No. 2	Average
SiO_2	63.45	62.77	63.110
Al_2O_3	23.04	23.56	23.300
Fe_2O_3	2.03	2.44	2.235
CaO	.72	.73	.725
MgO	1.03	.91	.970
Na_2O	.53	.45	.490
K_2O	.93	.93	.930
SO_3	.24	.24	.240
H_2O	7.81	7.81	7.810
	99.78	99.84	99.810

Two analyses of the glaze gave:

	(a)	(b)	Average
SiO_2	55.60	55.35	55.475
Al_2O_3	21.14	21.54	21.340
Fe_2O_3	2.54	2.74	2.640
CaO	3.55	3.45	3.500
MgO	.04	.04	.040
Na_2O	16.91	17.51	17.210
K_2O	.08	.08	.080
	99.86	100.71	100.285

The glaze gives the following formula:

$$\left.\begin{array}{l} .812\ Na_2O \\ .002\ K_2O \\ .182\ CaO \\ .002\ MgO \\ \hline .998\ RO \end{array}\right\} \left.\begin{array}{l} .612\ Al_2O_3 \\ .048\ Fe_2O_3 \end{array}\right\} 2.704\ SiO_2$$

The clay reduced to a formula becomes:

$$\left.\begin{array}{l} .056\ CaO \\ .105\ MgO \\ .034\ Na_2O \\ .042\ K_2O \end{array}\right\} \left.\begin{array}{l} 1.00\ Al_2O_3 \\ .06\ Fe_2O_3 \end{array}\right\} 4.605\ SiO_2\ (+\ 1.957\ H_2O)$$

The glaze reduced to the same type of formula as the clay, viz., $Al_2O_3 = 1$, enables the following comparison to be made:

$$\text{(Clay)} \quad \left.\begin{array}{l} .056\ CaO \\ .105\ MgO \\ .034\ Na_2O \\ .042\ K_2O \end{array}\right\} \begin{array}{l} 1.00\ Al_2O_3 \\ .06\ Fe_2O_3 \end{array} \left\{ 4.605\ SiO_2 \right.$$

$$\text{(Glaze)} \quad \left.\begin{array}{l} 1.326\ Na_2O \\ .003\ K_2O \\ .297\ CaO \\ .003\ MgO \end{array}\right\} \begin{array}{l} 1.00\ Al_2O_3 \\ .078\ Fe_2O_3 \end{array} \left\{ 4.418\ SiO_2 \right.$$

It is seen that the silica is a little higher in the body than in the glaze, the alumina being the same. This is not to be regarded as strange, as the salt fumes cannot be expected to attack all the constituents of the clay with equal ease and incorporate them into a glass in exactly the same proportions as they exist in the clay. A certain per cent in the clay of coarse free sand which the salt does not easily attack, would no doubt lower the silica content of the glaze below that of the clay. Or, an aluminous coal-ash would tend to increase the proportion of Al_2O_3 to SiO_2 in the glaze over that in the clay, by being carried up into the kiln with salt vapors and lodging in the glaze.

A noticeable and interesting difference is in the amount of lime in body and glaze. While the clay shows only .725 percent of lime, the glaze shows 3.5 per cent. The most reasonable explanation for this difference seems to be that of efflorescence. The fact that this clay contains soluble sulphates, and is, moreover, known to efflorescence, might easily explain the presence of so much lime in the glaze. The soluble salts were brought to the surface and left there as a coating, long before the salt was applied to the fires. On salting the kiln, this efflorescence (mostly $CaSO_4$) was naturally absorbed as a part of the glaze.

While it is seen that the ratios of alumina to silica in clay and glaze are not exactly the same, still they are very close. From this we may reasonably infer that the ratio of alumina to silica in any clay which can be salt-glazed, is a good index of approximately what this ratio will be in the salt-glaze itself.

Langenbeck* holds that the molecular ratio of alumina to silica in a salt-glaze should be about 1 to 8, and assuming that a clay to be properly salt-glazed should contain alumina and silica in a similar ratio, he states that a clay best suited for salt-glazing will contain its alumina and silica in a molecular ratio of about 1 to 8.

This ratio of Langenbeck's is not borne out by the above analyses, nor by subsequent actual tests made by the writer. While it was found that a clay containing alumina and silica in the ratio of 1 to 8 would salt-glaze well, it was also found that the ratio could be much lower or much higher, and an equally good glaze could be obtained.

THE EXPERIMENTS PROJECTED.

To attain the object of this paper, namely, to throw some light on the relation between the constituents of a clay and its ability to be successfully salt-glazed, two series of experiments were first taken up, one decreasing the ratio of alumina to silica over that shown by the clay analysis, by additions of sand; the other increasing the ratio of alumina to silica, by additions of kaolin.

Starting with the Logan clay unadulterated, systematic additions of sand were made until a ratio of alumina to silica of 1 to 10 had been reached. This, it was thought, would show a point where the silica would be too high for a good salt-glaze.

This series is shown by the following table, in which the composition of the various mixtures proposed is indicated by their molecular formulæ:

SERIES I.

No.	KNaO	CaMgO	Al_2O_3	Fe_2O_3	SiO_2	Ratio Al_2O_3 to SiO_2	Remarks
8	.076	.161	1.00	0.06	4.60	1 : 4.6	
9	"	"	"	"	5.00	1 : 5.0	
10	"	"	"	"	6.00	1 : 6.0	
11	"	"	"	"	7.00	1 : 7.0	
12	"	"	"	"	8.00	1 : 8.0	
13	"	"	"	"	9.00	1 : 8.0	
14	"	"	"	"	10.00	1 : 10.0	

* Chemistry of Pottery, p. 90.

For increasing the ratio of alumina to silica, the Logan clay was treated with increasing additions of Florida kaolin. A series was made starting with the Logan clay unadulterated, (in which the ratio of alumina to silica is 1 to 4.6,) and ending with trials made wholly of kaolin, with a ratio of alumina to silica of 1 to 2. This series was as follows:

SERIES II.

No.	KNaO	CaMgO	Al_2O_3	Fe_2O_3	SiO_2	Ratio Al_2O_3 to SiO_2	Remarks
8	.076	.161	1.00	0.06	4.60	1 : 4.60	
7	"	"	2.00	"	6.60	1 : 3.30	
6	"	"	3.00	"	8.60	1 : 2.86	
5	"	"	4.00	"	10.60	1 : 2.65	
4	"	"	6.00	"	14.60	1 : 2.45	
3	"	"	10.00	"	22.60	1 : 2.26	
2	"	"	20.00	"	42.60	1 : 2.13	
1	"	"	1.00	"	2.00	1 : 2.00	

The complete test covered by the two series is shown in the following table:

No.	KNaO	CaMgO	Al_2O_3	Fe_2O_3	SiO_2	Ratio Al_2O_3 to SiO_2	Remarks
1	....	....	1.00	....	2.00	1 : 2.00	Florida kaolin
2	.076	.161	20.00	0.06	42.60	1 : 2.13	Obtained by mixing Florida kaolin and Logan F. C.
3	"	"	10.00	"	22.60	1 : 2.26	
4	"	"	6.00	"	14.60	1 : 2.45	
5	"	"	4.00	"	10.60	1 : 2.65	
6	"	"	3.00	"	8.60	1 : 2.86	
7	"	"	2.00	"	6.60	1 : 3.30	
8	.076	.161	1.00	0.06	4.60	1 : 4.60	Logan F. C.
9	"	"	"	"	5.00	1 : 5.00	Obtained by additions of Flint to Logan F. C.
10	"	"	"	"	6.00	1 : 6.00	
11	"	"	"	"	7.00	1 : 7.00	
12	"	"	"	"	8.00	1 : 8.00	
13	"	"	"	"	9.00	1 : 9.00	
14	"	"	"	"	10.00	1 : 10.00	

INGREDIENTS FOR THE SYNTHETIC CLAYS.

The sand used in the above mixtures was glass sand, of 40 to 60 mesh fineness.

A set of brickettes was also made running from 9a to 14a inclusive, using 150 to 200 mesh sand, but retaining the same mixtures as in the 9 to 14. A third set was also made from 9b to 14b, using sand which would not settle in twenty minutes in a standard sedimentation can in the university laboratory.

The finest sand, that which would not settle in twenty minutes, was obtained by vigorously stirring up commercial potters' flint with water in the standard sendimentation can.

This can was about twenty-six inches high and twelve inches in diameter, with a spout or tubulature one and one-fourth inches in diameter, placed six inches from the bottom.* After allowing the flint and water to stand twenty minutes, the liquid was let off through the spout. This was repeated until practically all of the sand which would not settle in twenty minutes was drawn off. The milky liquid thus obtained was allowed to stand for several days, after which the clear water was drawn off and the fine flint was obtained and dried.

The 40 to 60 mesh was obtained by taking glass sand and throwing same on a 40 mesh screen; all that went through was subsequently thrown on a 60 mesh sieve, and all that remained on the 60 mesh was reserved for use. That which went through 60 or did not go through 40 was rejected.

The 150 to 200 mesh was obtained by taking the sand rejected in the preceding process and grinding in a ball mill for two hours, then putting through a 150 mesh and then a 200 mesh sieve. All that went through the 150 mesh and lodged on the 200 mesh was reserved for use; that which went through 200 was rejected, and that which did not pass through 150 was reground.

The kaolinite used was a Florida kaolin, 98 per cent. pure kaolinite. This was passed through a 20 mesh screen before use.

The following table gives the proportion of each ingredient used in making the brickettes:

* See Trans. Am. Cer. Soc., Vol. II, p. 104.

No.	Logan Clay Grams	Kaolin Grams	Sand		
			40 to 60 mesh	150 to 200 mesh	Floated
1	0.0	800.0	.....	.....	.....
2	60.5	739.5	.....	.....	.....
3	118.0	682.0	.....	.....	.....
4	190.0	610.0	.....	.....	.....
5	273.0	527.0	.....	.....	.....
6	350.0	450.0	.....	.....	.....
7	487.0	313.0	.....	.....	.....
8	800.0	0.0	.....	.....	.....
9	755.0	.....	55.0	.....	....
10	744.0	.....	156.0	.....	.....
11	662.5	.....	237.5	.....	.....
12	596.5	.....	303.5	.....	.....
13	543.0	.....	357.0	.....	.....
14	498.0	.....	402.0	.....	.....
9A	755.0	.....	.....	55.0	.....
10A	744.0	.....	.....	156.0	.....
11A	662.5	.....	.....	237.5	.....
12A	596.5	.....	.....	303.5	.....
13A	453.0	.....	.....	357.0	.....
14A	498.0	.....	.....	402.0	.....
9B	755.0	.....	.....	.....	55.0
10B	744.0	.....	.....	.....	156.0
11B	662.5	.....	.....	.....	237.5
12B	596.5	.....	.....	.....	303.5
13B	543.0	.....	.....	.....	357.0
14B	498.0	.....	.....	.....	402.0

PREPARING THE SAMPLES.

Two test brickettes were made from each mixture. These brickettes were made in a wooden mould 2½ x 4½ x 1 inches. The ingredients in each case were mixed thoroughly in a granite-ware pan, then made into a stiff mud with distilled water. The brickettes were dried in the laboratory by simply putting on pallets on shelves.

After drying the brickettes they were placed in No. 12 kiln of the Hocking Fire Clay Company, at Logan, Ohio. This is a round down draft kiln in which they regularly do salt-glazing. One brickette of each number was placed well to the top of the kiln, and one of each number put four inches above the floor. The kiln was burned and salted in the usual manner, and with results of ordinary attainment.

RESULTS OF BURN.

No. 8 brickettes, made from the Logan clay, took a nice smooth salt-glaze, deep brown in color, similar to the usual run of glazes obtained at this plant. This will be used as a basis of comparison.

Very little difference was noticed between the samples burned at the top and those burned at the bottom of the kiln. The results of the test were practically the same for both sets of brickettes, and were as follows:

The brickettes below No. 8 (those in which the ratio of alumina to silica was increased) deteriorated rapidly in the quality of the glaze. No. 7 was almost as good as No. 8, but the glaze was a little inferior in finish. Nos. 5 and 6 had a very poor dull glaze, greatly inferior to that of No. 8, and not at all entitled to be called a successful salt-glaze. No. 6 was a little better than No. 5. No. 4 had taken a dull reddish-brown gloss with insufficient thickness, glossiness or finish to be called a glaze. No. 3 had lost still further the resemblance to a glaze, and a dull red surface, slightly glossy in spots was all that the salt had accomplished. Nos. 1 and 2 had merely been colored a brick red, and showed no glaze whatever. No. 1 had the appearance of a red face brick, having assumed a fine red color very uniformly.

This red color shown by the unglazed brickettes is probably due to the volatilization of Fe_2Cl_6 and the subsequent deposition of Fe_2O_3, which occurs in salt-glazing, as shown by Knett,* and its entering afterwards into silicate formation with the clay. Occasionally red clouds of Fe_2Cl_6 appear at the beginning of the operation of salt-glazing a kiln, but soon cease. It is this Fe_2Cl_6 which is deposited on the ware as Fe_2O_3. The surface of the brickettes showing this color presented a beautiful mass of crystals under the glass.

Of the brickettes above No. 8 (those in which the ratio of alumina to silica was gradually diminished) all had taken a good salt-glaze. The following facts were noted:

* Thonindustrie-Zeitung (1896).

(*a*) As to brightness and smoothness, the brickettes from No. 8 up to No. 14 showed no deterioration, but on the other hand an improvement. No. 14 was a much brighter glaze than No. 8. In this respect the different finenesses of sand made no apparent difference, all finenesses for each number being equally bright and smooth.

(*b*) In color, the glazes grew much lighter as the sand content increased. Each brickette, from No. 8 up, was a little lighter than its predecessor. From No. 8, which was a deep brown, the color lightened to a stone gray in No. 14. A noticeable fact was that in the brickettes containing the finer sands, the lightening effect was increased. For instance, No. 11-A was as light as No. 14, and in the A series the color reached its lightest, being a fine clear gray in No. 14-A. In the B series No. 10-B was about the same shade as No. 11-A, and from No. 10-B the color grew lighter, but with a greenish tinge so that, while No. 14-B was very light it was a greenish gray and not a clear gray, as in No. 14-A.

(*c*) As to the crazing of the glazes, the following list gives the crazed and uncrazed glazes:

No.	Uncrazed	Crazed
8	8A 8B	....
9	9A 9B	.....
10	.....	10A 10B (slight)
11	.....	11A 11B
12	....	12A 12B
13	.....	13A 13B } slight
14	.....	14A 14B } slight

As the foregoing series showed no deterioration of the salt-glaze with the increase of silica, contrary to what was expected, it was decided to still further increase the ratio of silica to alumina. For this purpose the following series was made:

SERIES III.

No.	KNaO	CaMgO	Al_2O_3	SiO_2	Ratio Al_2O_3 to SiO_2	Remarks
15	.076	.161	1.00	12.50	1 : 12.5	
16	"	"	"	15.00	1 : 15.0	
17	"	"	"	20.00	1 : 20.0	
18	"	"	"	40.00	1 : 40.0	
19	"	"	"	60.00	1 : 60.0	
20	0.0	0.0	0.0	100.00	all sand	

In view of the fact that in the previous test the fineness of sand made no apparent difference in the quality of the glaze the piece took; and also that samples in the bottom of the kiln showed no wide difference from those in the top, it was deemed sufficient to use merely one grade of sand in Series III, and to make only one set of brickettes. Accordingly, the fine floated silica was used, and one set of brickettes made as follows:

No.	Parts Logan Clay	Parts Sand
15	367	433
16	313	487
17	242	558
18	127	673
19	86	714
20	0	800

The brickettes were molded as those in Series I, except that owing to the extreme shortness of the mixtures, due to so much sand, it was necessary to use dextrine as a bonding agent to stick the material together and make brickettes which could be handled.

The brickettes were placed well to the top in one of the salt glazing kilns of the Hocking Clay Manufacturing Company, and burnt with a kiln of their ware. On drawing, the following results of the test were noted:

No. 15 was glazed but the glaze was thin, and in some places grew so thin as to be dull and rough. The glaze could not be called a good salt-glaze.

No. 16 showed glassy and smooth in places, but was mostly dull and rough, with insufficient glassiness to be called glazed.

No. 17 showed faint traces of glaze, but the greater part of the surface was dull, and while the salt had affected the entire surface, it had merely made a coating not unlike melted sugar in appearance. No. 17 presented a very interesting point, in that the brickette was well glazed on top but on none of the sides. No doubt, as the piece was exposed to flying ash and dust in the kiln, a certain amount of coal ash lodged on top of the brickette. This coal ash supplied the necessary alumina and the brickette the necessary silica to make a salt-glaze. The ash being unable to cling to the vertical sides of the brickette left these portions too silicious to make a glaze.

Nos. 18, 19 and 20 showed no traces of glaze whatever, and the salt had had no action on the pieces whatever. These brickettes had the same finish to their surface as when they entered the kiln, and very much resembled sand or bath bricks.

From Series I it was seen that a clay with an alumina-silica ratio of 1 to 10 gave a good salt-glaze. From Series III it was found that a clay with an alumina-silica ratio of 1 to 12.5 did not give a good salt-glaze. The limit lies between these two ratios, but lack of time prevented its exact determination. In view of this we shall have to assume temporarily that the limit was 1 to 12.5. This is more than likely, as the ratio of 1 to 10 gave a very good glaze, and the ratio of 1 to 12.5, while not a good glaze exactly, still it was not far over the line.

The following conclusions may now be safely drawn from the foregoing experiments in regard to the alumina-silica ratio.

1. A clay may be either too aluminous or too silicious to be successfully salt-glazed.

2. Clays containing alumina and silica between the molecular ratio of

1.00 alumina to 4.6 of silica
1.00 " " 12.5 "

are capable of receiving a salt-glaze if the process is properly carried out. If these limits are exceeded the material is not suited for salt-glazed ware.

3. While it is possible to salt-glaze clays containing alumina and silica in the above ratios, the requirements of the process of making stiff-mud goods would not permit the use of such silicious clays as those with an alumina-silica ratio of 1 to 9 or 1 to 10. That is, the practical value of a clay, as to plasticity, strength, etc., will draw narrower limits than those found in the above tests.

4. As regards brightness, smoothness and finish of the salt-glaze, it makes but little difference whether the free silica in the clay is fine or coarse.

5. As regards color, the finer the sand the lighter the color of the glaze. This has been substantiated in a practical way by Mr. A. Magoon, Superintendent of the Hocking Clay Manufacturing Co.

INFLUENCE OF EFFLORESCENCE.

Having come to some conclusion as to the limits of alumina and silica in a clay with reference to its receiving a good salt-glaze, it was decided to next look a little into the matter of efflorescence, or the presence of soluble salts in a clay and their bearing on the salt-glazing question.

Referring again to the difference in the lime contents of the Logan clay and the salt-glaze, as shown by the analyses, it is to be noted in this case that whatever efflorescence comes to the surface of the clay in burning did not interfere seriously with the clay taking a good glaze. The analysis of the glaze indicates that soluble salts were absorbed and became a part of the glaze.

Professor Orton * speaks of lime salts in paving brick interfering with their being salt-glazed, and many practical sewer-pipe burners will tell you of the troublesome "scum" which makes salt-glazing harder to accomplish.

The question now arises as to what extent the salt-vapors are able to attack efflorescence and secure the needed

* Ohio Geol. Survey, Vol. VII, Pt. I, p. 176.

alumina and silica from the clay beneath, and what amount of soluble salts in a clay will interfere seriously with salt glazing it.

In order to make an experiment in this direction some of the Logan fire clay was treated repeatedly with distilled water and finally with hot distilled water, the water being siphoned off after each treatment until it was judged that whatever soluble salts the clay contained had been dissolved out and drawn off in the water. The clay was then dried and ground, and put through a 20 mesh sieve. Starting with this clay soluble salts were added systematically up to three percent. The soluble salts added were calcium sulphate and magnesium sulphate in the proportion of three parts of the former to one of the latter.

The series was as follows:

SERIES IV.

No.	Logan Clay free from Soluble Salts	$CaSO_4$	$MgSO_4$	Per Cent. Soluble Salts
1-E	796	3.00	1.00	0.5
2-E	792	6.00	2.00	1.0
3-E	788	9.00	3.00	1.5
4-E	784	12.00	4.00	2.0
5-E	780	15.00	5.00	2.5
6-E	776	18.00	6.00	3.0

Two sets of these brickettes were made, one set was kept in the laboratory to dry, in order to note the appearance of efflorescence, if any, in drying. The other set was placed while damp in one of the Logan kilns, and burned.

The set of brickettes dried in the laboratory showed merely traces of efflorescence which did not increase with the amount of soluble salts contained in the clay.

The brickettes burned and salt-glazed all showed a good, smooth, deep brown glaze. The brightness of the glaze increased with the amount of soluble salts. The color of the glaze in each case was remarkable for its rich brown tone, being a deeper color than is usually obtained in the regular run of goods from these kilns.

Whether the soluble salts which had been added to the clay were brought to the surface in burning, is a question which cannot be decided with the data at hand. The set put in the laboratory to dry, however, showed that the efflorescence did not come out to any extent in drying.

The effect of efflorescence therefore is still a matter to be decided. That enough calcium sulphate can exist on the surface of a brick to prevent its taking a good salt glaze under standard conditions, the writer has demonstrated by heavily coating a number of green bricks at the Logan factory with plaster of paris mixed with water, and placing same in different parts of the kiln. The surfaces which were treated in this manner came out slightly glazed, but with a dull yellowish, unsightly color.

SALT GLAZE CONSIDERED AS A GLASS.

Knett gives the following as the reactions which occur in salt-glazing:

$$(Al\ Fe)_2O_3 \times SiO_2 + 6\ Na\ Cl = (Al\ Na_3)_2O_3 \times SiO_2 + Fe_2Cl_6$$
$$Fe_2Cl_6 + 3\ H_2O = Fe_2O_3 + 6\ HCl$$

According to this, the ratio of soda to alumina in the glaze would be

$$3\ Na_2O : Al_2O_3$$

Langenbeck * says, "Such glazes (salt-glazes) approximate a formula:

$$1\ RO,\ .5\ R_2O_3,\ 4.\ SiO_2$$

By referring to formula III, deduced from the actual analysis of the Logan salt-glaze, namely:

$$\left.\begin{array}{l} 1.326\ Na_2O \\ .003\ K_2O \\ .297\ CaO \\ .003\ MgO \end{array}\right\} \left.\begin{array}{l} 1.000\ Al_2O_3 \\ .078\ Fe_2O_3 \end{array}\right\} 4.418\ SiO_2$$

It is readily seen that both Knett and Langenbeck place the ratio of alumina to soda much lower than that found by the writer to actually exist in the glaze.

In order to prove the feasibility of glasses high in alumina, it was decided to make up a series of glasses to substantiate the glaze formula found by analysis.

*Chemistry of Pottery.

In a series of glass experiments Professor Orton * demonstrates that a glass of the following composition:

$$\left.\begin{matrix}.5\ Na_2O\\ .5\ CaO\end{matrix}\right\} 0.20\ Al_2O_3,\ \ 0.80\ SiO_2 \qquad \text{Oxygen Ratio } 1:1$$

was much more fusible at cone 8 than a glass of the composition:

$$\left.\begin{matrix}.5\ Na_2O\\ .5\ CaO\end{matrix}\right\} 0.0\ Al_2O_3,\ \ 0.50\ SiO_2 \qquad \text{Oxygen Ratio } 1:1$$

Starting with the glass given in the above formula, it was decided to not merely increase the alumina as Professor Orton had done, but to replace the lime with alumina in order to approach the salt-glaze formula, which is practically:

$$\left.\begin{matrix}.8\ Na_2O\\ .2\ CaO\end{matrix}\right\} 0.6\ Al_2O_3,\ \ 2.70\ SiO_2 \qquad \text{Oxygen Ratio } 1:1.93$$

To this end the following series of glasses was made up:

SERIES V.

No.	Na_2O	CaO	Al_2O_3	SiO_2
1	0.5	0.5	0.0	0.5
2	0.5	0.4	0.033	0.5
3	0.5	0.3	0.066	0.5
4	0.5	0 2	0.100	0.5
5	0.5	0.1	0.133	0.5
6	0.5	0.0	0.166	0.5

In this series the oxygen ratio was kept constant, namely, 1 : 1.

These glass mixtures were placed in porcelain cups and fired in a test kiln to cone No. 8. On cooling and drawing the kiln the following results were noted:

(*a*) No. 1 was a stony mass, resembling blast furnace slag.

(*b*) Nos. 2 to 6, inclusive, were all good glasses, well melted.

(*c*) No. 5 was a little better glass than No. 6, showing lime had helped the mixture to some extent.

(*d*) No. 5 was as good, if not a little better than any of the other glasses.

* Trans. Am. Cer. Soc., Vol. IV.

Taking the formula for No. 5 glass, namely:

$$\left.\begin{matrix}.5\ Na_2O \\ .1\ CaO\end{matrix}\right\} .133\ Al_2O_3,\ \ 0.50\ SiO_2$$

and reducing this to a basis of RO=1, we obtain

$$\left.\begin{matrix}.83\ Na_2O \\ .17\ CaO\end{matrix}\right\} .221\ Al_2O_3,\ \ .83\ SiO_2$$

It will be seen that the RO contents here are very close indeed to the RO contents of the Logan salt-glaze.

Having proved that a soda-alumina glass is fully as feasible as a soda-lime glass, and having obtained in the No. 5 glass an RO content practically the same as in the salt-glaze, a series was next prepared, increasing the alumina and silica contents of No. 5 glass and also the oxygen ratio, until the formula for the salt-glaze was obtained.

The series for this purpose was as follows:

SERIES VI.

Number	Na_2O	CaO	Al_2O_3	SiO_2	RO	Remarks
7	0.80	0.20	0.221	.83	1 : 1.00	
8	"	"	0.360	1.30	1 : 1.25	
9	"	"	0.440	1.76	1 : 1.50	
10	"	"	0.510	2.23	1 : 1.75	
11	"	"	0.600	2.70	1 : 1.93	Logan Salt-Glaze

This series was treated as the preceding, being melted in porcelain cups at cone No. 8.

On cooling and drawing the following results were noted:

(*a*) All had melted to glasses.

(*b*) Nos. 7, 8, 9 and 10 had melted into solid cakes of glass, showing very little difference.

(*c*) Nos. 7 and 8 showed no bubbles in the glass. No. 9 showed a number of small bubbles through the glass mass, and No. 10 showed larger bubbles. But these were still very good glasses.

(*d*) No. 10 while a glassy mass, very much the same in color as the Logan salt-glaze, was very spongy throughout. The mass, however, was a good, sharp, well melted glass, which, if ground up and applied to a brick surface, would

undoubtedly flow to a glaze very similar to the salt-glaze. The bubbling was merely due to insufficient time in melting to allow the carbon gases to completely escape.

By the foregoing glass experiments it has been clearly demonstrated.

First. That in a soda-lime glass similar to that shown by No. I, Series V, alumina may replace the lime in part or even completely, and a good glass will still be obtained. In other words, soda-alumina glass is as feasible as a soda-lime glass at the same heat.

Second. That a little lime in a glass mixture of this type makes a little better glass than with no lime.

Third. That a soda-lime alumina glass can be made at cone No. 8 with a wide range of alumina and silica. Glassy masses are obtained with mixtures in which the alumina and silica runs from 0.221 Al_2O_3, 0.83 SiO_2 to 0.600 Al_2O_3, 2.70 SiO_2.

Fourth. That by compounding a mixture corresponding to the formula deduced from the analysis of the salt-glaze, a a glass is obtained at salt-glazing temperature (cone No. 8), very similar in all respects to the salt-glaze actually obtained in commercial work. The analysis of the salt-glaze and the subsequent compounding of a glass from this analysis therefore harmonize perfectly.

SUMMARY.

In the above paper, we have obtained the analysis and formulæ for both the clay and glaze actually used in successful salt-glazing work. From the experiments it was found that clays with a wide molecular ratio of alumina to silica could be salt-glazed, viz.: from 1 : 4.6 to 1 : 12.5. Also, from the glass tests, we found that glasses could be made with a generous variation of alumina and silica. Connecting these observations, it is reasonable to suppose that the salt-glaze on the different clays varied in composition according to the clay composition. For instance, in the Logan clay and glaze, the ratio of alumina to silica is very close. Now when the ratio in the clay had been changed, is it not probable that the composition of the glaze formed changed, also ?

This can only be determined by actually analysing the salt-glaze formed on different bodies of known composition.

By the efflorescence test it was demonstrated that up to three per cent. of soluble salts may be present in clay without seriously interfering with salt-glazing when conducted at cone No. 8. Whether or not, however, all the added salts came to the surface was not determined.

It would be an interesting point for other investigators in this line, to determine the effect of sulphurous gases on salt-glazes. This could be done by treating a piece of salt-glazed ware with sulphur gases in a closed tube. That the action of sulphur has an important bearing on the quality of the salt-glaze seems highly probable. It would be of interest and value to also determine the temperature at which glazing begins, and whether it can be overdone by high heat. Also to ascertain the relation of the temperatures of salt-glazing red-burning clays and buff-burning clays, and what effect $CaSO_4$ or other soluble salts has on the glazing of red-burning clays. Furthermore, whether steam aids the process or not is still a questionable point.

The writer regrets that the time set apart for this work on salt-glazing did not permit of his looking into the last mentioned points, but hopes that they will be investigated before long.

In conclusion, the writer desires to thank especially Mr. A. Magoon, of the Hocking Clay Manufacturing Company, who generously took the time and trouble to furnish samples of his clay and glaze, and to take care of the burning of the experimental trials.

THE CONSTITUTION OF CHROMIUM-TIN PINK.

BY
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The subject of the chromium-tin colors offers a field for investigation to which, considering the extent to which the colors are used in ceramic work, but little attention has been paid. Most of the published information on the subject we owe to Leykauf and Seger. The former studied the compounds of chromium and tin from a purely chemical standpoint, while Dr. Seger has given receipts and directions for the production of ceramic colors by the use of these materials.

REVIEW OF PREVIOUS INVESTIGATIONS.

Leykauf concluded from his investigations, (Jour. Soc. Chem. Ind., 1892) that chromium stannate can be obtained by igniting stannic chromate at a high temperature, and forms a dark violet mass, from which glazes varying in color from rose-red to dark violet can be prepared. He found that when chromic oxide and stannic oxide were ignited together in the proportion of one of the former to fifty of the latter, that a mass composed of a mixture of fine crystals and fused, glassy particles of a beautiful permanent lilac color were obtained. According to Leykauf, "pink color" consists of this substance, mixed with calcium stannate.

Leykauf's statement that "pink color" contains chromic stannate was contradicted by Petrik, who found that a pink coloring body could be produced as well by the aid of alumina or a mixture of alumina and magnesia, as by means of tin oxide. He used for the purpose the precipitated hydroxides obtained from 90.6 g. ammonia alum, and 24.6 g. magnesium sulphate, which were ground together with .5 g. potassium chromate. The mass was burned twice, being well washed after the first burning. During the second burning the red color appeared. In the presence of a larger amount of chromic oxide, only a green color resulted. An increase in the amount of magnesia favored the formation of the red color, while the substitution of lime, baryta or strontia for magnesia

seemed to have a contrary effect. Petrik thought that this was the case because the precipitated alumina was coarser, and that, in all probability, the chromium color compound only appears pink when very finely divided. From this investigation, Petrik concluded that the "pink color" does not contain chromic stannate, but that a red chromium compound, apparently chromic oxide in a finely divided state, is precipitated upon the stannic acid serving as a "base."

The investigations of Leykauf and Petrik are of considerable interest, but the hypotheses which they offered in explanation of the formation of the color are evidently contradictory, and the facts produced in each case were insufficient to establish any theory.

If we lay aside the theoretical considerations and look for the useful facts which have been published on the subject, we find them stated briefly in Seger's discussion of underglaze colors, as follows:

"Chromium oxide with tin oxide and lime produces red tints. The red colors show many peculiarities which must be considered. They are obtained, when to a mixture of marble and tin oxide a small addition of potassium chromate is made, and the mixture strongly ignited. Outside of the potassium chromate, quite a number of substances produce the same effect, but it is safest to work with potassium chromate. However, if too much of the latter is used, (over 3 to 4 per cent,) the resisting power of the underglaze color is reduced considerably. If silica is added to the mixture the color turns toward red, if the lime is decreased, toward lilac, and excluding the latter from the composition entirely, it becomes a pure lilac color. A very strong fire is necessary. The red colors require oxidizing conditions, and are destroyed by reduction but renewed on continued oxidation."

In addition to this general discussion, Seger gives, with directions for their preparation, the following receipts:

Pink-Red.

	Parts
Pure ignited tin oxide	50
Marble	25
Quartz	18
Acid potassium chromate	3
Borax	4
	100

This fritt, after ignition, would have the following formula:

$$\left.\begin{array}{l} .924\ CaO \\ .038\ K_2O \\ .038\ Na_2O \end{array}\right\} .038\ Cr_2O_3 \left\{\begin{array}{l} 1.109\ SiO_2 \\ 0.076\ B_2O_3 \\ 1.238\ SnO_2 \end{array}\right.$$

Reddish-Blue.

	Parts
Ignited tin oxide	70
Marble	23
Acid potassium chromate	3
Borax	4

This fritt, after ignition, would have the following formula:

$$\left.\begin{array}{l} .918\ CaO \\ .0406\ K_2O \\ .0414\ Na_2O \end{array}\right\} .0406\ Cr_2O_3 \left\{\begin{array}{l} .0828\ B_2O_3 \\ 1.875\ SnO_2 \end{array}\right.$$

By leaving out the marble, a lilac is obtained which is suitable for white ware, but cannot resist a high heat like that of the porcelain kiln.

Lilac.

	Parts
Ignited tin oxide	50
Potassium bichromate	3
Borax	20

of which the formula would be:

$$\left.\begin{array}{l} .163\ K_2O \\ .837\ Na_2O \end{array}\right\} .163\ Cr_2O_3 \left\{\begin{array}{l} 1.674\ B_2O_3 \\ 5.36\ SnO_2 \end{array}\right.$$

The first of these receipts is used by many potters, not only as an underglaze color, but also in majolica glazes. However, in some cases considerable difficulty is experienced in using it, owing to the fact that the glazes destroy the color. An example of this sort of difficulty, and the manner in which it was overcome, is found in the paper by Mayer, in the Transactions of the American Ceramic Society, Vol. I. Mr. Mayer's experiments proved conclusively that in his case the trouble was caused by the glaze, and that another glaze could be used, which, under the same conditions, did not destroy the underglaze color. Why one glaze destroyed the color and another did not, has not been determined.

SCOPE OF THIS INVESTIGATION.

The experiments described below were made for the purpose of adding something to the available information upon the following phases of this subject: *First.* The most

favorable composition for the pink fritt. *Second,* The type of low fire glaze which develops the color to the best advantage.

THE MOST FAVORABLE COMPOSITION OF THE PINK FRITT.

The work done on the first phase of the subject, consisted of making three series of fritts, beginning with a series of combinations of chromic oxide and stannic oxide alone. Taking the members of this series which had the highest allowable ratio of chromic oxide to stannic oxide, a second series was made, keeping this chrome-tin ratio constant, and adding an increased proportion of lime. Finally, the best member of the chrome-tin-lime series was taken as a basis for a third series, in which silica was added in regularly increasing proportions.

In these series, the tin was supplied by SnO_2, the chromium by CrO_3 and the lime by $CaCO_3$. These materials were all chemically pure, but for the silica, Golding flint was used. The SnO_2, $CaCO_3$ and SiO_2 were all fine enough to make further grinding unnecessary, the coarsest leaving about 3 per cent. on an 80 mesh screen. In making the mixtures, the insoluble ingredients for each fritt were weighed dry, and placed in a caserole. The CrO_3 was added from a burette, a standard solution being used. Sufficient water was then added to make the mass into a thin paste, which was thoroughly stirred and then dried on a water bath, the stirring being repeated at frequent intervals to insure an intimate mixture of the ingredients. Each mixture was then placed in an air bath to complete the drying, and finally each was thoroughly ground in an agate mortar. Owing to the fineness of the materials used, it was possible to secure a good fine mixture with a reasonable amount of grinding, and it is thought that by adding CrO_3 in solution, a better contact of the particles was secured than could have been done by a dry process of mixing.

The fritts were calcined in a small gas furnace, fired by means of a blast lamp. The furnace consists of a vertical hollow cylinder of fire clay, the inside diameter being about five inches and the height six inches. The blast lamp flame

enters horizontally at the bottom of the furnace. The cylinder is covered with a fire clay disc, in the center of which is an inch hole which serves the double purpose of a peep hole and gas outlet. With this furnace we were able to reach cone 18 with oxidizing conditions. The fritts, about 30 g. each, were calcined in small fire-clay crucibles without lids. Each member of the first two series was fired for forty-five minutes, at about cone 16. The third series was kept at cone 8 to 10 for an hour.

The following table shows the composition of the members of the first series:

SERIES A-I.

No. of Fritt	Composition by Weight		Equivalents.	
	Parts of SnO_2	Parts of CrO_3	SnO_2	Cr_2O_3
1	50	1	1	.015
2	40	1	1	.0185
3	30	1	1	.025
4	20	1	1	.0375
5	15	1	1	.050
6	10	1	1	.075
7	5	1	1	.150
8	2.5	1	1	.300

The appearance of the fritts of the first series, after calcination, were as follows:

Each fritt was in a soft friable mass, none of them having formed a hard cake. The color of the first four was a rich purple, nearly as strong in one as in the other. Beginning with No. 4, there came a regular gradation of color, ranging from a rich purple to the green of Cr_2O_3. This showed that with these conditions the maximum allowable molecular ratio of Cr_2O_3 to SnO_2 was .0375 : 1. Above this ratio the excess of SnO_2 was not great enough to prevent the existence of free Cr_2O_3.

No. 4 of this series was allowed to cool very slowly in order to favor the formation of crystals, and was then examined microscopically. The microscope revealed isolated white particles and colored masses, which showed no distinct granular or crystalline particles. When viewed through crossed Nicol's prisms, no evidence of crystallization could

be detected. This does not correspond to the description given by Leykauf. It is true that the white particles looked like crystals, but they failed to show any evidence of polarization.

For the second series, the ratio of SnO_2 to CrO_3 was maintained the same as that in No. 4 of the first series, and the lime was increased as shown in the following table:

SERIES A-II.

No. of Fritt	Percents by Weight		Molecular Proportions		
	Fritt No. 4	$CaCO_3$	SnO_2	CaO	Cr_2O_3
9	100	0	1	.0	.0375
10	80	20	1	.391	.0375
11	60	40	1	1.04	.0375
12	40	60	1	2.34	.0375
13	20	80	1	6.25	.0375

The effect of the lime upon the chrome-tin fritt was very marked. No. 10 was a reddish-brown, while No. 11 was a stronger, deeper brown. The higher numbers became glassy, and the color took on a greenish cast, which predominated in the highest numbers. No. 13 melted to a green glass, which attacked the crucible strongly.

Taking the chrome-tin-lime ratio of 11, the effect of SiO_2 was then tried by the following series:

SERIES A-III.

No. of Fritt	Molecular Proportions			
	SnO_2	$CaCO_3$	SiO_2	Cr_2O_3
14	1	1	.0	.0375
15	1	1	.5	.0375
16	1	1	1.0	.0375
17	1	1	1.5	.0375
18	1	1	2.0	.0375

In this series the colors ranged from a strong brown in No. 14 to a strong pink in No. 18. The difference in shade between Nos. 16, 17 and 18 was, however, very slight, and any of these would pass for a good pink fritt.

It would require a much more extended investigation than this to determine to what extent the proportions of these fritt ingredients could be varied, and a good fritt be obtained. The following consideration made it seem desirable to use, in the glazes to be tried, a formula not differing widely from that of the upper numbers of Series A-III:

1. The tin might be raised without detriment to the fritt itself, but it is not considered desirable to introduce more tin than is necessary into soft fire glazes. It may be that the tin could be decreased, but the results of these experiments do not indicate that this could be done to any considerable extent.

2. Series A-I indicates that the quantity of chromic oxide is as high as is allowable.

3. The lime was purposely made as high as Series A-II would warrant.

4. This fritt corresponded closely, both in appearance and in composition to the pink fritt recommended by Seger, except that the tin is lower in the fritts of Series A-III. Seger's fritt, however, was intended for an underglaze color for porcelain, and in that case a high excess of tin in the fritt would not be detrimental to the glaze.

THE EFFECT OF LEAD AND BORACIC ACID UPON THE CHROMIUM-TIN PINK IN LOW FIRE GLAZES.

As has been stated, it was desired to determine what type of low fire glaze is most favorable for the development of the pink color. In planning this investigation, the following facts were to be taken into consideration:

1. Too high a lead content in the glaze is detrimental to the color. Professor Orton, in one of his lectures on the subject of colored glazes, states that in a chromium-tin pink glaze not more than .45 equivalents of lead is permissible. Moreover, preliminary tests of my own with a glaze containing .6 equivalents of lead failed to produce a pink glaze at all.

2. Seger states that boracic acid in the fritt changes the color toward purple, and it was expected that boracic acid in the glaze would produce the same effect to some extent.

In view of these facts the problem seemed to resolve itself into the following phases: *First*, To determine the permissible content of lead. *Second*, To find how much B_2O_3 is allowable. *Third*, To design a glaze maturing at cone 02 or 01, in which these limits of composition are not exceeded.

The first thing which was attempted in this connection was to determine the effect of increasing the content of boracic acid in a glaze containing lead. The glaze selected for this purpose had the following formula:

$$\left.\begin{array}{l}0.25\ PbO\\0.25\ KNaO\\0.50\ CaO\end{array}\right\}\ 0.2\ Al_2O_3,\ 2.00\ SiO_2\ \left\{\begin{array}{l}0.005\ Cr_2O_3\\0.185\ SnO_2\end{array}\right.$$

It was thought that .25 equivalents of lead was not sufficient to be detrimental to the color, and that quantity was accordingly used to help in softening the glaze.

The boracic acid was introduced into the glaze by the use of a fritt of the following composition:

$$\left.\begin{array}{l}0.5\ Na_2O\\0.5\ CaO\end{array}\right\}\ 0.054\ Al_2O_3\ \left\{\begin{array}{l}2.29\ SiO_2\\1.19\ B_2O_3\end{array}\right.$$

The pink fritt used in this series was the underglaze pink recommend by Seger:

$$\left.\begin{array}{l}0.924\ CaO\\0.038\ K_2O\\0.038\ Na_2O\end{array}\right\}\begin{array}{l}1.109\ SiO_2\\0.076\ B_2O_3\end{array}\ \left\{\begin{array}{l}1.288\ SnO_2\\0.038\ Cr_2O_3\end{array}\right.$$

The composition of the glazes of this series is shown by the following table:

SERIES B.

No.	PbO	KNaO	CaO	Al_2O_3	SiO_2	B_2O_3	Cr_2O_3	SnO_2
1	.25	.25	.50	.2	2.00	.35	.005	.185
2	.25	.25	.50	.2	2.00	.40	.005	.185
3	.25	.25	.50	.2	2.00	.45	.005	.185
4	.25	.25	.50	.2	2.00	.50	.005	.185

It will be seen that the boracic acid varies between somewhat narrow limits in this case, but the range proved to be wide enough for the purpose.

The method of preparation of these glazes and of all those which will be discussed, was as follows:

The batches for the first and last glaze of each series were weighed out and ground wet in porcelain ball mills. After grinding, the glaze batches were screened through a 150 mesh screen. As most of the materials were finely ground to start with, two or three hours of grinding were sufficient in all cases, to make the glaze slip go through the 150 mesh sieve like so much water. After screening, the slip was allowed to settle and the supernatant water syphoned off as closely as possible. A sample of each slip was then taken and the quantity of dry material per unit of slip determined. This completed the data necessary to calculate the proportions in which the two slips would have to be blended to produce the intermediate numbers of the series.

The manner of the preparation of the fritts, except in the case of the pink fritts, for which a description has already been given, was to grind the fritt batch dry to secure a good mixture, and melt it in a large Hessian crucible in the gas furnace already described. The fritt when melted, was poured from the crucible into cold water, replacing and refilling the crucible and repeating the operation as many times as necessary. Fritts could be poured in this way which would not readily run through an opening in the bottom of the crucible. After being crackled in this way, a fritt would be ground wet in a glaze mill, screened through 150 mesh sieve, allowed to settle and the water syphoned off. The fritt was then dried ready for weighing.

The burning, unless otherwise stated, was done in a down-draft test kiln. With this kiln it was possible to get a fairly uniform distribution of the heat and to make as good a burn as can be expected with a test kiln having but one furnace. Of course, there were short periods of reduction, when fresh fuel was added, but this by careful firing could be kept from going too far. Coke was used for fuel in each burn until a red heat was reached. At that stage the use of coal was commenced. However, no fresh coal was thrown upon the grate bars, but was placed upon the coking plate just inside the furnace door and kept there till most of the volatile part of the fuel was roasted out before spreading

over the bars. In this way the condition of the kiln gases was kept fairly uniform.

The glazes were all applied to white tiles, 4½ inches long by 1 inch in width and set in saggers, which were then covered and luted tight.

The trials of series B were set on end in a large sagger and placed near the top of the kiln. Cones were placed in front of the sagger in a smaller sagger placed on its side. In burning, the heat was raised as rapidly as it could be and still keep the predominating conditions of the kiln strongly oxidizing. Cone 04 was reached in eleven hours. The heat was brought up more slowly during the next hour and the firing discontinued at the end of the twelfth hour, with cone 02 down in the front sagger, 01 having been reached in the sagger with the trials.

In the resulting glazes, the effect of the boracic acid was apparent. As was expected, the boracic acid softened the glaze, but also seemed detrimental to the color.

The appearance of the individual pieces was as follows:

Glaze B-1.

B_2O_3=.35—This glaze was somewhat immature and showed a tendency to bead up, leaving patches of bare surface on the tile. Slight tendency to flow. The color was a deep, strong maroon.

Glaze B-2.

B_2O_3=.40—Glaze fairly well matured, with some tendency to flow. Glaze would pass aside from color. Color, slightly inferior to B–1, there being some white flocky segregations which are detrimental. Shade nearly the same as that of B–1.

Glaze B–3.

B_2O_3=.45—This is a good nature glaze but its appearance is bad. The flocky segregations, apparently due to tin, predominate.

Glaze B–4.

B_2O_3=.50—In this glaze the pink is almost entirely destroyed and the tin-enamel effect is solid instead of flocky.

It is evident that in this glaze, the use of more than .4 equivalents of boracic acid is not permissible. It is also evident that, under the conditions, at least .4 B_2O_3 is required in order that the glaze will mature. Moreover, the color is not bright enough to be satisfactory in any of the pieces.

The only definite conclusion that can be drawn from this series is that this particular glaze, (B–1) does not develop a good pink color when softened with sufficient B_2O_3 to make it mature under the conditions described at cone 0I.

In order to determine more definitely the effect of different equivalents of PbO and B_2O_3 upon the color, three series of glazes were made, being so designed as to keep the acid ratio and the alumina content constant throughout. In one case, SiO_2 alone was used for the acid, and in the RO group the ratio of KNaO to CaO was kept approximately 1 : 2, while the content of PbO increased regularly by additions of .2 equivalents. In the other series the RO group was maintained constant, while the SiO_2 was replaced by successive additions of .2 equivalents of B_2O_3. Finally a series was made in which the acid ratio was kept constant, but the PbO was made to decrease, while the B_2O_3 increased. The extremes for these series were as follows:

1. A fritted soda-lime glaze free from both PbO and B_2O_3:

$$\left.\begin{matrix}0.33\ KNaO\\0.67\ CaO\end{matrix}\right\}\ 2.00\ Al_2O_3,\ \ .2\ SiO_2\ \left\{\begin{matrix}0.005\ Cr_2O_3\\0.144\ SiO_2\end{matrix}\right.$$

In making this glaze, a fritt of the following composition was used, obviating the use of much raw material, and thereby softening the glaze considerably:

$$\left.\begin{matrix}0.33\ KNaO\\0.67\ CaO\end{matrix}\right\}\ 0.18\ Al_2O_3,\ 1.00\ SiO_2$$

2. A raw lead glaze without B_2O_3:

$$\left.\begin{matrix}0.851\ PbO\\0.011\ KNaO\\0.138\ CaO\end{matrix}\right\}\ 0.2\ Al_2O_3,\ 2.00\ SiO_2\ \left\{\begin{matrix}0.005\ Cr_2O_3\\0.144\ SnO_2\end{matrix}\right.$$

In this glaze, the CaO and KNaO were only such as were necessarily brought in by the pink fritt.

3. A fritted boracic acid glaze without lead:

$$\left.\begin{matrix}0.33\ KNaO\\0.67\ CaO\end{matrix}\right\}0.2\ Al_2O_3\ \left\{\begin{matrix}1.00\ SiO_2\\1.00\ B_2O_3\end{matrix}\right\}\begin{matrix}0.005\ Cr_2O_3\\0.144\ SnO_2\end{matrix}$$

In the batch for the glaze the amount of raw material was reduced to .05 equivalents of clay, by using the following fritt:

$$\left.\begin{matrix}0.378\ KNaO\\0.622\ CaO\end{matrix}\right\}0.176\ Al_2O_3\ \left\{\begin{matrix}.858\ SiO_2\\1.170\ B_2O_3\end{matrix}\right.$$

In these three series a pink fritt of the following composition was used:

$$\left.\begin{matrix}0.924\ CaO\\1.109\ SiO_2\\0.038\ K_2O\\0.076\ B_2O_3\\0.038\ Na_2O\end{matrix}\right\}\begin{matrix}1.238\ SnO_2\\0.038\ Cr_2O_3\end{matrix}$$

The following tables contain the formulæ of the glazes obtained by blending the extremes:

SERIES C.

No.	PbO	KNaO	CaO	Al_2O_3	SiO_2	Cr_2O_3	SnO_2
1	.000	.330	.670	.2	2.0	.005	.144
2	.200	.254	.544	.2	2 0	.005	.144
3	.400	.180	.420	.2	2.0	.005	.144
4	.600	.105	.294	.2	2.0	.005	.144
5	.851	.011	.138	.2	2.0	.005	.144

SERIES D.

No.	KNaO	CaO	Al_2O_3	SiO_2	B_2O_3	Cr_2O_3	SnO_2
1	.33	.67	.2	2.0	0.0	.005	.144
2	.33	.67	.2	1.8	0.2	.005	.144
3	.33	.67	.2	1.6	0.4	.005	.144
4	.33	.67	.2	1.4	0.6	.005	.144
5	.33	.67	.2	1.2	0.8	.005	.144
6	.33	.67	.2	1.0	1.0	.005	.144

SERIES E.

No.	PbO	KNaO	CaO	Al_2O_3	SiO_2	B_2O_3	Cr_2O_3	SnO_2
1	.851	.011	.138	.2	2.0	0.0	.005	.144
2	.681	.075	.244	.2	1.8	0.2	.005	.144
3	.511	.139	.350	.2	1.6	0.4	.005	.144
4	.340	.208	.456	.2	1.4	0.6	.005	.144
5	.170	.266	.563	.2	1.2	0.8	.005	.144
6	.000	.330	.670	.2	1.0	1.0	.005	.144

The glazes of these series were prepared, dipped, set and burned as described for series B.

The results obtained, while they failed to give the color that was desired, demonstrated unmistakably the effects of lead and boracic acid in the glaze.

Series C showed a regular gradation from a fairly good pink color in C–1 to a glaze in C–5, in which there was no resemblance whatever to a pink color. Individually, the glazes were as follows:

Glaze C–1.

PbO=0—Glaze immature. Color pink, but dull, owing to immaturity of glaze. This same glaze when fired to cone 3, gave a good clear pink.

Glaze C–2.

PbO=.2—Very bright and pretty pink enamel over greater portion of surface, surrounded by a border of white enamel. The glaze is somewhat immature. The same glaze was burned at cone 3 and though over-burned, gave a strong pink color with a fringe of white.

Glaze C–3.

Pbo=.4—Strangely enough, this number shows more of a tendency to bead up and have bare spots on the surface of the tile. The appearance is that of an immature enamel, fringed with white, with a flush of pinkish brown over the remainder of the surface. The shade is much darker than in C–2.

Glaze C–4.

PbO=.6—A gray opaque enamel, beaded to some extent. It is found by breaking the piece so as to show it in cross-section, that this enamel is mature only at the surface, beneath which the color is not destroyed.

Glaze C–5.

PbO=.851—Very ugly enamel with a tinge of green. From this series it is evident in the first place that none of the glazes will mature at cone 01 with the sort of burn described; and in the next place that for this glaze the upper limit of PbO lies somewhere between .2 and .4 equivalents. In this series, as also in the one preceding it, the

quantity of pink fritt was too great. The colors, where not destroyed, were unnecessarily strong, the glazes too opaque, and naturally more flux was required to produce a mature glaze than would be necessary if less of the pink fritt were used. It is thought that C–2 and C–3 would be considerably improved by using only enough pink fritt to introduce .003 instead of .005 equivalents of Cr_2O_3.

In Series D we have the effects of B_2O_3 even more clearly demonstrated. The glazes change progressively from immature pinks in D–1 through a series of mature purplish pinks, to a dark purple glaze, which is very immature at the other extreme.

Glaze D–1.

Same as C–1.

Glaze D–2.

B_2O_3=.2—This glaze is somewhat immature. The color though not fully developed, is a deep strong maroon. The same glaze fired to cone 3 resulted in a good smooth glaze with a strong pink color, slightly tinted with purple.

Glaze D–3.

B_2O_3=.4—This glaze is badly scummed but is perfect in other respects. The color is a strong dark pink with a marked purplish tint.

Glaze D–4.

B_2O_3=.6—Glaze not quite mature. In appearance very bad. It has a mottled effect produced by flocky seggregations of a pale purple color, replacing the pink. In this glaze the B_2O_3 is evidently too high.

The next two numbers, D–5 and D–6, are both far beyond the limit. Both are immature, and a very dark purple in color.

D–3 is by far the best number in this series. On one side of D–3 in the series, the lower limit of B_2O_3 is marked by D–2 with .2 B_2O_3, while on the other side the upper limit is passed before D–4 with .6 B_2O_3 is reached.

This series leads us to the following conclusions:

1. That B_2O_3 gives a purple tint to the pink color.
2. That in this glaze the quantity of B_2O_3 that can be

used is below .6 equivalents. The results of this series are consistent with those of series B, in which the best glaze contained .4 B_2O_3. The softening effect of the fritting in series D is obtained in series B by the use of .25 PbO. The color is better in the glaze without lead, and yet the comparison indicates that as much B_2O_3 is permissible in one case as in the other.

Series E. In view of the results obtained in Series C and D it is apparent, that no member of Series E can be satisfactory; for as we go down the list in the table, we see that before we get below the upper limit of PbO, we pass above the upper limit of B_2O_3, and therefore the limit of either PbO or B_2O_3 is exceeded in every number of the series. E–4, whose composition is above the limit in both PbO and B_2O_3. but not far above either, would naturally be selected as the nearest approach of any to a favorable combination.

An inspection of this series shows that the actual results are exactly what we would expect, and are therefore entirely consistent with the preceding series. E–1, E–2 and E–3 resemble C–5 and C–4 which are also high in lead, while E–5 and E–6 are almost identical with D–5 and D–6, containing respectively the same equivalents of B_2O_3. E–4 is not so good as D–4, owing to its lead content. This makes the series consistent throughout.

We may therefore conclude that in glazes of the types employed in these tests:

1. A high lead content destroys the color of the chromium-tin pink.

2. A high content of boracic acid is also detrimental to the pink color, and boracic acid in any quantity gives the color a purple tint.

3. The allowable limit of PbO is below .4 equivalents.

4. The limit of B_2O_3 is below 6. equivalents.

In view of these results, it is apparent that with a glaze of the type employed up to this point, a lead glaze without boracic acid will not answer the purpose: for not only does

a high lead content destroy the pink color, but it also produces a glaze which beads up and will not lie smooth, and the higher the lead content is made, the worse the glaze behaves. Moreover, with a leadless glaze, while it is possible to produce the desired effect at cone 3 or 4 by the use of boracic acid, we find that when sufficient boracic acid is used to mature the glaze at cone 01, the color effect is destroyed. Neither lead nor boracic acid alone is able to soften the glaze—

$$RO, 0.2\ Al_2O_3,\ 2.00\ SiO_2 \left\{ \begin{matrix} 0.005\ Cr_2O_3 \\ 0.144\ SnO_2 \end{matrix} \right\}$$

enough to make it mature at cone 01 without being detrimental to the color. Nevertheless, since the effects produced by the lead and boracic acids were opposite in character, it was thought that as much or more of each could be used with the other as without it, combining their softening effects, and neutralizing the detrimental tendencies of each other.

It was therefore decided to make another series, in which both the lead and boracic acid should be regularly increased, in order to determine to what extent each could be used in connection with the other. But before making these tests, it was considered desirable to determine whether it would be feasible to reduce the refractoriness of the glaze by decreasing the ratio of CaO to KNaO, or by decreasing the acidity of the glaze. It was also decided to reduce the quantity of pink fritt in the glaze, as this is one of the refractory agents.

The effect of decreasing the lime-soda ratio was tried by the use of the following series:

SERIES F.

No. of Glaze.	KNaO	CaO	Al_2O_3	SiO_2	Cr_2O_3	SnO_2	Ratio (CaO:KNaO)
1	.333	.666	0.2	2.00	0.003	0.087	2.00:1
2.	.400	.600	"	"	"	"	1.50:1
3	.500	.500	"	"	"	"	1.00:1
4.	.600	.400	"	"	"	"	0.66:1
5.	.666	.333	"	"	"	"	0.51:1

In making F-1, the following fritt was used:

$$\left.\begin{matrix}0.33\ KNaO\\0.67\ CaO\end{matrix}\right\}0.13\ Al_2O_3,\ 1.00\ SiO_2$$

The raw materials added to this fritt to make the glaze were:

Spar	.04	Equivalents
Clay	.047	"
Flint	.698	"

The glaze batch contained over 50 per cent of the lime-soda fritt.

For F-5, a fritt was designed as follows, leaving only .07 equivalents of raw clay to add to the glaze batch:

$$\left.\begin{matrix}0.726\ KNaO\\0.24\ CaO\end{matrix}\right\}0.148\ Al_2O_3,\ 1.94\ SiO_2$$

As a consequence, some allowances should be made in considering these results, for the fact that toward the latter end of this series, a greater proportion of the glaze mixture is fritted and that this tends to reduce the refractoriness somewhat. This difference was probably not great enough to be of much consequence, however.

These glazes were fired in the up-draft test kiln, cone 5 being reached in about fourteen hours. The resulting glazes showed the following effects:

As the ratio of CaO to KNaO decreased, the fusibility increased somewhat, but F-5 was the only one which was fully mature. However, with the increase of fusibility there was an increasing tendency for the glazes to bubble even though immature. The last ones of the series were rough at the edges, while smooth over the surface of the tile. As to color, the detrimental effects of the lower lime-soda ratio was not so great as had been anticipated, yet the color degenerated steadily from a bright pink in F-1 toward brown in F-5.

After making this test, it was decided to continue the use of the hightest ratio — CaO: KNaO=2:1, because the softening effect of the lower ratio was not considered great enough to compensate for the detrimental effect on the color.

Having fixed on a lime-soda ratio, another series was made to determine whether a reduction in acidity would

produce a less refractory glaze. In order that all the conditions except the acid ratio should remain constant in this series, all the ingredients of both extremes were fritted. The series was as follows:

SERIES G.

No. of Glaze	KNaO	CaO	Al_2O_3	SiO_2	Cr_2O_3	SnO_2	Acid Ratio
1.	0.333	0.666	0.200	2.00	.003	.087	2.50
2.	"	"	0.170	1.70	"	"	2.25
3.	"	"	0.148	1.48	"	"	2.00
4.	"	"	0.118	1.18	"	"	1.75
5.	"	"	0.097	0.97	"	"	1.50

This series, like series F, was fired to cone 5 in about fourteen hours. The result was not at all in favor of a lower ratio than 2.5. This glaze, G-1, which is the same as C-1, D-1, and F-1 was slightly immature, as was expected, but of fairly good color. However, as the ratio decreased, no good glazes were produced, all being immature, rough and badly blistered. The color, also, became poorer. It was decided, therefore, that for lime-soda glaze at least the acidity was none too high, and the type glaze RO, .2 Al_2O_3 2 SiO_2 was retained for the remaining series.

As has been stated, the following set of series was designed to demonstrate the combined effects of lead and boracic acid when used together. In each series the lead content was kept constant, while the B_2O_3 was regularly increased by increments of .1 equivalent from nothing up to .5 equivalent, beyond which it was judged unnecessary to go.

The lead content of the first series was placed at .15 equivalent, and each successive series contained an addition of .10 equivalent, making the lead content of the fourth series .45 equivalent, which was supposed to be above the limit.

The series were as follows:

SERIES H.

No.	PbO	CaO	KNaO	Al_2O_3	SiO_2	B_2O_3	Cr_2O_3	SnO_2
1	0.15	0.566	0.283	0.2	2.0	0.0	0.003	0.087
2	"	"	"	"	1.9	0.1	"	"
3	"	"	"	"	1.8	0.2	"	"
4	"	"	"	"	1.7	0.3	"	"
5	"	"	"	"	1.6	0.4	"	"
6	"	"	"	"	1.5	0.5	"	"

SERIES I.

No.	PbO	CaO	KNaO	Al_2O_3	SiO_2	B_2O_3	Cr_2O_3	SnO_2
1	0 25	0.50	0.25	0.2	2.0	0.0	0.003	0.087
2	"	"	"	"	1.9	0.1	"	"
3	"	"	"	"	1.8	0.2	"	"
4	"	"	"	"	1.7	0.3	"	"
5	"	"	"	"	1.6	0.4	"	"
6	"	"	"	"	1.5	0.5	"	"

SERIES J.

No.	PbO	CaO	KNaO	Al_2O_3	SiO_2	B_2O_3	Cr_2O_3	SnO_2
1	0.35	0.433	0.216	0.2	2.0	0.0	0.003	0.087
2	"	"	"	"	1.9	0.1	"	"
3	"	"	"	"	1.8	0.2	"	"
4	"	"	"	"	1.7	0.3	"	"
5	"	"	"	"	1.6	0.4	"	"
6	"	"	"	"	1.5	0.5	"	"

SERIES K.

No.	PbO	CaO	KNaO	Al_2O_3	SiO_2	B_2O_3	Cr_2O_3	SnO_2
1	0.45	0.366	0.183	0.2	2.0	0.0	0.003	0.087
2	"	"	"	"	1.9	0.1	"	"
3	"	"	"	"	1.8	0.2	"	"
4	"	"	"	"	1.7	0.3	"	"
5	"	"	"	"	1.6	0.4	"	"

The glazes of these series were made by blending the three following glazes:

1. A raw lead glaze nearly as high in lead as the lime and alkalies, introduced by the pink fritt would permit:

$$\left.\begin{array}{l}0.800\ PbO\\0.133\ CaO\\0.066\ KNaO\end{array}\right\}0.2\ Al_2O_3,\ 2.0\ SiO_2\left\{\begin{array}{l}0.003\ Cr_2O_3\\0.087\ SnO_2\end{array}\right.$$

2. A fritted boracic acid glaze:

$$\left.\begin{matrix}0.666\ CaO\\0.333\ KNaO\end{matrix}\right\}0.2\ Al_2O_3\left\{\begin{matrix}1.0\ SiO_2\\1.0\ B_2O_3\end{matrix}\right.\left\{\begin{matrix}0.003\ Cr_2O_3\\0.087\ SnO_2\end{matrix}\right.$$

This glaze was made without any raw ingredients, using the pink fritt and one of the following composition:

$$\left.\begin{matrix}0.641\ CaO\\0.359\ KNaO\end{matrix}\right\}0.2\ Al_2O_3\left\{\begin{matrix}0.991\ SiO_2\\1.100\ B_2O_3\end{matrix}\right.$$

In all the Series except A and B, the pink fritt given for Series C has been used.

3. A fritted lime-soda glaze, made exactly as described for F-1, as follows:

$$\left.\begin{matrix}0.666\ CaO\\0.333\ KNaO\end{matrix}\right\}0.2\ Al_2O_3,\ 2.00\ SiO_2\left\{\begin{matrix}0.003\ Cr_2O_3\\0.087\ SnO_2\end{matrix}\right.$$

With these three glazes it was possible to make blends covering all combinations within the limits assigned. The calculations were made as follows: Suppose I-4 were to be made. This glaze contains .25 equivalent PbO and 0.3 B_2O_3. The lead extreme contains .8 equivalents PbO; therefore, we must use $0.25 \div 0.80 = .312$ equivalents of the lead glaze. For the B_2O_3 we take $0.3 \div 1 = .3$ equivalent of the boracic acid glaze. This make $0.312 + 0.3 = .612$ equivalent, leaving $1.000 - 0.612 = .388$ equivalent of the lime-soda glaze. Knowing the equivalent weight of each glaze, we have but to multiply each decimal by the corresponding equivalent weight for the batch weight.

These glazes were placed on end in saggers, which were then luted tight and placed in the down draft test kiln. The kiln was burned in about fourteen hours, cone 1 being reached. The results can be stated best by describing the individual pieces:

Series H. PbO=0.15.

H-1. Immature, rather pale pink with a margin of white enamel.

H-2. Glaze slightly darker and more nearly mature than H-1.

H-3. Glaze fused enough to develop a strong dark color, but not enough to run smooth. Scummed.

H-4. Glaze mature and color well developed but shows a tendency to segregate. Has run too much, leaving the color too thin near the top.

H–5 and 6. As the B_2O_3 is raised the tendency to flow increases, leaving the color too thin on the tile. The glazes tend toward greenish, but segregations of pink remain.

Series I. $PbO=0.25$.

I–1. Glaze beaded and immature, color inferior. White margin.

I–2. Still beaded but somewhat smoother and better color.

I–3. Glaze fairly smooth and of good color. Has run a little, leaving color rather thin and uneven near top. Scummed slightly.

I–4. Glaze mature and brilliant. Has run freely and the color is not dense enough near the top in consequence.

I–5 and 6. Pale glazes mottled with pink segregations. Pronounced green tint in No. 6.

Series J. $PbO=0.35$.

J–1. Badly beaded, poor color, white margin. Same tendencies as in I–1, but carried to a greater extent.

J–2. Beaded, most of the tile bare, but color fairly well developed.

J–3. Glaze nearly mature, color fair except at margin. Glaze has not run.

J–4. Mature. Has run considerably, small portions of the tile bare, color fair but not well distributed—similar to H–5 and I–5.

J–5. About like I–6.

J–6. Greenish glaze, mottled with pink.

Series K. $PbO=0.45$.

K–1. Beaded worst of all but the color not destoyed.

K–2. Still bad.

K–3. Not yet smooth, some spots bare, others thick. Pale margin at top.

K–4. Glaze mature and fairly smooth. Has run considerably. Color uneven and inferior.

K–5. Pale green glaze with only a faint trace of pink.

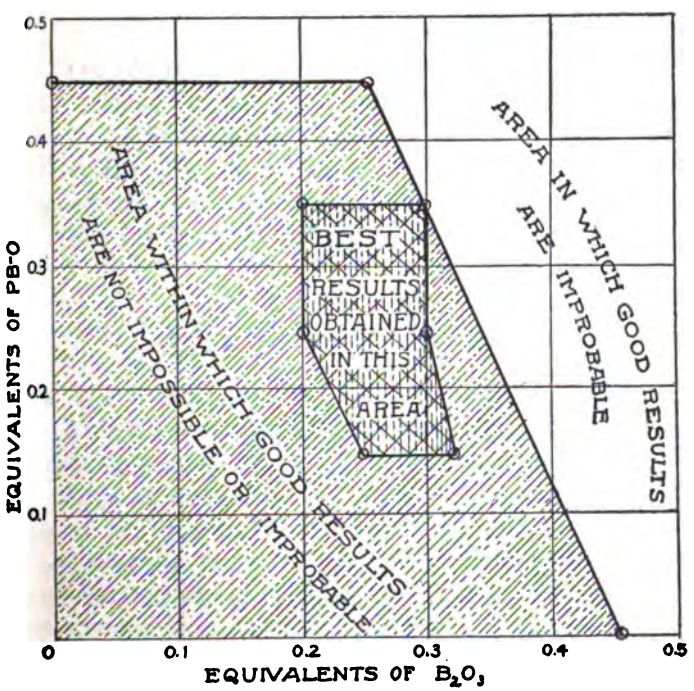
A review of these results brings out the following points:

1. In producing a chromium-tin pink glaze at low temperature, it is best to use both lead and boracic acid.

2. The more lead the glaze contains, the more boracic acid is required to overcome the tendency to bead; but the greater the lead content in the glaze, the less boracic acid can be used without destroying the color. This places a limit on the allowable lead content. These results indicate that this limit is below .45 equivalent of PbO.

3. Although more boracic acid can be used in glazes low in lead than in those with a high lead content, yet, even in leadless glazes, too much boracic acids destroys the pink color. As stated before, this limit is below 0.6 equivalent B_2O_3—probably below 0.5 B_2O_3.

The following diagram roughly defines the limits of PbO and B_2O_3 when used together for this purpose in a glaze of this type; all those combinations within the curve may be considered permissible so far as color is concerned, and may be expected to mature at some temperature. The most favorable combinations for cone .01 to cone 1 are within the cross-hatched area.



THE BEHAVIOR OF THE DIFFERENT PINK FRITTS IN THE SAME GLAZE.

These series of pink fritts have already been described. To these was added a fourth series, showing the effect of lime in the fritt.

SERIES A-IV.

No.	CaO	SnO_2	SiO_2	Cr_2O_3
19	0.00	1.00	2.00	0.0375
20	0.25	"	"	"
21	0.50	"	"	"
22	0.75	"	"	"
23	1.00	"	"	"
24	1.50	"	"	"
25	2.00	"	"	"
26	3.00	"	"	"

These fritts were prepared in a manner similar to that described for the previous fritt series. The appearance of the fritts was as follows:

19. Pale and inferior color with a greenish tint.

20. Somewhat stronger pink.

21. The color improves up to 22 with 0.75 CaO. From 22 upward, the color runs steadily toward brown, and the powder shows an increasing tendency to vitrify.

A glaze was now selected in which the different fritts were to be incorporated, in order to demonstrate the coloring effect of each in the glaze. The glaze used for the purpose was the following:

$$\left.\begin{array}{l}0.25\ PbO\\0.50\ CaO\\0.25\ KNaO\end{array}\right\}0.2\ Al_2O_3\left\{\begin{array}{l}1.75\ SiO_2\\0.25\ B_2O_3\end{array}\right.$$

This glaze was prepared, and a portion of slip equivalent to forty five grams of dry glaze was weighed out for each fritt. Five grams of each fritt to be tested were then weighed out and each portion ground wet in an agate mortar and screened through a 200 mesh sieve into the receptacle containing the corresponding portion of glaze slip. The glaze was then stirred thoroughly to distribute the fritt through it. These glazes were then dipped, set and burned to cone 1, as described for the last set of series preceding.

The results may be described as follows:

The fritts of Series A-I, made from chromic acid and stannic oxide alone did not color the glaze well. No. 2 produced purple specks in the glaze, but poorly disseminated. In No. 4 the color was nearly destroyed, indicating that the proportion of chromic oxide was too high.

Of fritt Series A-II, containing 1 SnO_2, 0.0375 Cr_2O_3 and CaO varying, No. 10 with .391 CaO and No. 11 with 1.04 CaO were tried. Both gave good strong colors. The glaze of No. 10 was slightly scummed, while that of No. 11 was scummed badly and was less mature.

In Series A-III No. 15 produced a color not dissimilar to that produced by the Seger fritt, but somewhat lighter and perhaps a little better distributed. In 16 and 17 the color becomes weaker, owing to dilution of the fritt by silica.

The fritts of Series A-IV all produced fairly good colors. The glazes containing the fritts highest in lime, scummed worst, probably due to the fact that the higher lime content made the glazes more refractory.

The Seger fritt and my modification of the Seger fritt were also tried, and the effects were very similar to most of the others described.

These results indicate that a wide range in variation in the quantity of lime and silica used in the fritt is allowable, and that the color of the fritt is not a sure indication of the color which it will produce in a glaze. In this set of tests the best colors were produced by the fritts which contained no silica, but this was probably due to the fact that five grams of the flintless fritt represented a larger quantity of the other ingredients, and therefore had a greater coloring power. It seems that almost any combination of tin with a very small proportion of chromium, together with some lime or silica will make a fritt which is capable of producing a good pink color in glazes of the type—RO, 0.2 Al_2O_3, 2 SiO_2, provided that the ratio of lime to the alkalies in the glaze be kept high and the content of lead and boracic acid be kept within the limits assigned. It is not known to what chemical action the production of the pink color is due, but it is evident that while a high temperature is necessary for the

development of a fritt which will produce the pink color in a glaze, yet the action of the glaze at a comparatively low temperature, has the effect of changing the color of the fritt. The content of lime and the content of silica both have a marked effect on the color of the fritt, and yet a fritt containing silica and no lime, produces in the glaze a color similar to that developed by a fritt containing lime and no silica.

On the other hand, as has been demonstrated, a glaze too low in either lime or silica, is detrimental to the color. These observations lead one to emphasize the statement of Mr. Mayer, that in producing the different variations of this color, the greater part of one's attention should be paid to the composition of the glaze, and not to that of the pink fritt.

THE FLUXING POWER OF MICA IN CERAMIC BODIES.

BY

RAY THOMAS STULL, E. M., TERRA COTTA, ILL.

All clays contain mica, which is generally present in small amounts, but in some cases it comprises a considerable portion of the mass, sometimes reaching as high as 30 per cent.

The real effect of mica upon the refractory properties of a clay is not definitely known. At the present time but very little can be found in ceramic literature pertaining to the effect of mica in ceramic mixtures. Langenbeck, in his book entitled "The Chemistry of Pottery," speaks of mica as a flux, but gives no evidence as proof of this statement. "Technology of Ceramics," by Auscher et Quillard, says that mica should impart resistance and fusibility to a clay. What is here meant by resistance is rather vague, but probably refers to the strength of the body. The authors appear to be somewhat uncertain in regard to this statement, by using the word "should."

Ceramists hold that the fluxing value of feldspar is due to its alkali content. If the alkali is the real fluxing agent, then it seems reasonable to suppose that any other alkali bearing mineral would be equally good as a flux, in proportion to its alkali content. Others are of the opinion that a molecule of the fluxing mineral acts as a unit until the temperature is reached, at which fusion or decomposition of the mineral begins to occur. This fusion or decomposition may be brought about at a lower temperature by the action of some other substance upon it. This is generally the case of a flux acting upon a flux. In this way a high fire flux may be made to exert its fluxing power at a temperature far below that at which it would otherwise occur.

The white micas are divided into two groups, the alkali micas and the magnesian micas. The alkali micas form a group of minerals whose formulæ run somewhat parallel to those of the feldspars; the principal difference is that these

micas are generally higher in alumina. The close similarity in chemical construction of muscovite mica with that of orthoclase feldspar is, perhaps, one reason for their close association in nature. This close association accounts for the fact that the mica found in white clays is almost invariably the muscovite mica.

The formulæ given for orthoclase feldspar and muscovite mica are:

Orthoclase feldspar,	$K_2O, Al_2O_3, 6\ SiO_2$
Muscovite mica,	$K_2O, 3\ Al_2O_3, 6\ SiO_2, 2\ H_2O$

The similarities in composition are:

1. Each contains K_2O, Al_2O_3 and SiO_2.
2. Each contains one molecule of K_2O.
3. Each contains six molecules of SiO_2.

The dissimilarities in composition are:

1. Muscovite has $3\ Al_2O_3$ against $1\ Al_2O_3$ in orthoclase.
2. Muscovite has $2\ H_2O$ against none in orthoclase.

The ratio of K_2O to SiO_2 in each is 1 to 6. In so far as the K_2O is concerned, each mineral would have the same fluxing advantage. In orthoclase, the ratio of Al_2O_3 to SiO_2 is 1 to 6, while in muscovite the ratio is 3 to 6.

Seger found that by starting with pure alumina and making increased additions of silica, he could reduce the refractory qualities of the mixture up to a molecular ratio of Al_2O_3 to $SiO_2 = 1:17$, which gave him the most fusible combination of alumina and silica. On further additions of silica, the refractoriness of the mixture began to increase.

From this, the natural conclusion would be that the ratio, Al_2O_3 to $SiO_2 = 3$ to 6 in muscovite is more refractory than 1 to 6 in orthoclase. But, however, we cannot depend upon the theoretical assumption that the ratios of RO to SiO_2 and Al_2O_3 to SiO_2 determine the refractoriness, fluxing power or fusibility of a mineral when incorporated as a ceramic mixture, since the presence of other ingredients may materially change these qualities.

It was the object of the following work to determine, if possible, whether muscovite mica acted as a flux or whether it imparted refractory properties to a ceramic body, when fired at temperatures within the range for burning white

biscuit ware. Also, if it did have a fluxing power, how would it compare to that of orthoclase feldspar under the same conditions?

THE MATERIALS AND THEIR PREPARATION.

The materials used in this experiment were: 1 muscovite mica; 2 Brandywine feldspar; 3 Florida ball clay; 4 Golding flint.

The mica was the scrap variety from Jackson County, N. C. When received it ran in sizes from three inches across by one-half inch thick down to very small, thin flakes. In the laminae of some of the pieces were stains of iron. When observed, such pieces were rejected.

The mica was placed in a ball mill twenty-four inches in diameter by twenty-five inches in length. Three hundred and fifty pounds of Iceland flint pebbles were used, and sufficient water added to cover the balls three inches deep. The mica was allowed to grind for fifteen hours at thirty-six revolutions per minute. Owing to the tough nature of the mica, this long period of grinding was resorted to in order to reduce it as nearly as possible to the same degree of fineness as that of the feldspar, so that the two minerals should have the same advantages in fineness of grain. After grinding, the mica was removed from the mill, and screened through a laun of 120 meshes to the linear inch. That portion which remained on the laun was rejected, and represented less than 10 per cent. of the original batch. The portion which passed through the laun was placed in a tall can, allowed to settle over night, the supernatant liquid siphoned off, and the mica pulp evaporated to dryness.

The mica pulp when dry was nearly white in color, having a delicate light gray appearance. It was reduced to such a degree of fineness that the unaided eye could scarcely distinguish the fine mica flakes. On moistening with water it took on a dark gray appearance and developed a surprising degree of plasticity; in fact, it showed a greater degree of plasticity than some clays which are being worked successfully at the present time. This tends to prove that plasticity in kaolinite is due to fineness of grain and thin plate-like structure.

The commercially ground feldspar was taken from the bin, blunged thoroughly with water and screened through a 120 mesh laun in order to insure the proper degree of fineness, and to free it from woody fibre and other foreign material. But very little feldspathic residue was left on the laun. The portion passing through the laun was placed in a tall can, allowed to settle, decanted and dried in the same way as was done in the preparation of the mica.

The commercially prepared Florida ball clay, better known as Florida kaolin, was blunged to a thin slip and passed through the 200 mesh laun for the purpose of eliminating the foreign matter, and to take out as much of the mica as was possible to do. In this way over 0.5 per cent. of the weight of clay was removed as mica. The clay was allowed to settle, decanted and dried.

As the flint was known to be a very pure variety and of the proper degree of fineness, it was used from the bin without any extra preparation.

ANALYSES OF THE MATERIALS.

	Mica	Feldspar	Kaolin	Flint
SiO_2	66.14	66.39	45.69	99.23
Al_2O_3	17.57	19.13	39.28	....
Fe_2O_3	5.12	.19	.40	.51
CaO	1.20	.42	.90	....
MgO	.76	.12	.18	....
K_2O	5.31	12.01	.13	....
Na_2O	.99	1.83	.00	....
H_2O	3.02	.00	13.61	....
Total	100.11	100.09	100.19	99.74

The molecular formulæ calculated from these analyses, taking Al_2O_3 as unity, are:

MUSCOVITE, FORMULA NO. 1.

$$\left.\begin{array}{l} .1243\ CaO \\ .1103\ MgO \\ .3280\ K_2O \\ .0929\ Na_2O \end{array}\right\} \begin{array}{l} 1.0000\ Al_2O_3 \\ .1857\ Fe_2O_3 \end{array} \left\{ 6.399\ SiO_2\ (.974\ H_2O) \right.$$

$$.6555\ RO$$

Combining weight, 582.167

FELDSPAR, FORMULA NO. 2.

$$\left.\begin{array}{l} .0400\ CaO \\ .0160\ MgO \\ .6812\ K_2O \\ .1574\ Na_2O \end{array}\right\} \begin{array}{l} 1.0000\ Al_2O_3 \\ .0063\ Fe_2O_3 \end{array} \left\{ 5.900\ SiO_2 \right.$$

$$\overline{.8946\ RO}$$

Combining weight, 538.679

FLORIDA KAOLIN, FORMULA NO. 3.

$$\left.\begin{array}{l} .0417\ CaO \\ .0117\ MgO \\ .0086\ K_2O \end{array}\right\} \begin{array}{l} 1.0000\ Al_2O_3 \\ .0065\ Fe_2O_3 \end{array} \left\{ 1.977\ SiO_2\ (1.963\ H_2O) \right.$$

$$\overline{.0560\ RO}$$

Combining weight, 260.167

In the mica formula it will be observed that the molecular ratio of K_2O to Al_2O_3 is approximately 1 to 3, which corresponds to the theoretical formula for muscovite. Attention is also called to the high percent of silica in the mica. This may be accounted for by the wear upon the flint pebbles during the long period of grinding. The "Chemistry of Geology," Volume II, by Bischoff, gives as the limits of composition for mica:

SiO_2	36 to 71
Al_2O_3	6 " 38
Fe_2O_3	0 " 36
MgO	0 " 29
K_2O	2 " 14

Undoubtedly some silica was introduced by the wear upon the flint pebbles, but nevertheless it is more than likely that the mica was the highly silicious variety.

MAKING THE BODIES.

Two body series were made, Series A and Series B. Series A consisted of eleven bodies, first starting with 100 percent Florida kaolin, decreasing the kaolin each time by 10 per cent. and adding a corresponding amount of ground mica, up to all mica and no kaolin. These bodies were marked A-1, A-2, A-3, etc., up to A-11. The composition of Series A by weight is:

A-1	100	Kaolin	0	Mica
A-2	90	"	10	"
A-3	80	"	20	"
A-4	70	"	30	"
A-5	60	"	40	"
A-6	50	"	50	"
A-7	40	"	60	"
A-8	30	"	70	"
A-9	20	"	80	"
A-10	10	"	90	"
A-11	0	"	100	"

These bodies were made for the purpose of determining whether mica had any fluxing action upon kaolin, or if it had any vitrifying properties alone.

If mica exerted a fluxing power, it was also desirable to know how this fluxing power compared with that of feldspar in an ordinary whiteware body, and to what extent it could take the place of feldspar in such a body, and its effect upon the general appearance of the body.

For this purpose Series B was made. This was done by blending two bodies, each one having the same chemical formula. The only difference in these two bodies was that the first one called "S" was made from flint, kaolin and feldspar, while the second, which was marked "M," was made from kaolin, flint and mica, without the use of feldspar.

As a starting point for making this series, a well known whiteware body was selected which would give a comparatively wide range of vitrification, shrinkage and porosity, within the temperature range for burning ordinary white biscuit ware. The batch weights for this body are:

Florida Kaolin	50%
Golding Flint	38
Brandywine Spar	12

Considering the flint as pure SiO_2, and using the formulæ for the kaolin and feldspar according to their ultimate analyses, the formula for this body figures:

FORMULA NO. 4.

$$\left.\begin{array}{l} .0415\ CaO \\ .0121\ MgO \\ .0758\ K_2O \\ .0165\ Na_2O \\ \hline .1459\ RO \end{array}\right\} \begin{array}{l} 1.0000\ Al_2O_3 \\ .0065\ Fe_2O_3 \end{array} \left\{ 5.388\ SiO_2 \right.$$

Body "M" was made by taking the proper proportions of Florida kaolin and mica so as to furnish all the RO and Al_2O_3 in formula No. 4, and supplying the difference in SiO_2 by flint.

The calculation of Body M was as follows:

Let X = equivalent of kaolin
Let Y = " " mica

required to supply .1459 RO and 1.000 Al_2O_3. Then

.0574 X RO in kaolin + .6555 Y RO in mica = .1459 RO in formula 4
Also, X Al_2O_3 in kaolin + Y Al_2O_3 in mica = 1.000 Al_2O_3 in formula 4
X = .8514 equivalents of kaolin
and Y = .1486 " " mica

.8514 equivalents kaolin + .1486 equivalents mica give

$$.1459\ RO \left\{ \begin{matrix} 1.000\ Al_2O_3 \\ .033\ Fe_2O_3 \end{matrix} \right\} 2.623\ SiO_2$$

Then there remains 5.338—2.623 = 2.715 equivalents of flint to be added. The batch weights for body "M" are:

Kaolin	47.04%
Flint	34.59
Mica	18.37

Calculating the formula of this mixture as a check upon our operations, we find the following formula for body "M.',

FORMULA NO. 5.

$$\left.\begin{matrix} .0540\ CaO \\ .0263\ MgO \\ .0518\ K_2O \\ .0138\ Na_2O \\ \hline .1459\ RO \end{matrix}\right\} \left\{ \begin{matrix} 1.000\ Al_2O_3 \\ .033\ Fe_2O_3 \end{matrix} \right\} 5.338\ SiO_2$$

In supplying the total RO and Al_2O_3 in formula No. 4 by kaolin and mica alone, the Fe_2O_3 has been overburdened by an amount .033—.0065 = .0265. This equivalent of Fe_2O_3 was added to formula No. 4, giving formula No. 6 as the formula for body "S."

FORMULA NO. 6.

$$\left.\begin{matrix} .0415\ CaO \\ .0121\ MgO \\ .0758\ K_2O \\ .0165\ Na_2O \\ \hline .1459\ RO \end{matrix}\right\} \left\{ \begin{matrix} 1.000\ Al_2O_3 \\ .033\ Fe_2O_3 \end{matrix} \right\} 5.338\ SiO_2$$

In comparing formula No. 6 with that of No. 5, it is observed that the two have the same chemical equivalent of RO, and that they have the same Al_2O_3, Fe_2O_3 and SiO_2. In comparing the RO in No. 6 with that of No. 5, we see that No. 5 is higher in CaO and MgO, and correspondingly lower in K_2O and Na_2O, thus giving body "S" containing spar a slight advantage in fusibility over body "M" containing mica.

The batch weights for body "S" calculated from formula No. 6 are:

Kaolin	49.93%
Flint	37.81
Spar	12.16
Fe_2O_3	.10

Bodies "M" and "S" were each weighed from the dry materials, placed in separate porcelain lined ball mills and ground wet for thirty minutes. The two batches were then removed, screened through a 100 mesh screen, and placed in two separate cans. The percent of solid material per unit weight of slip was determined for each, and Series "B" was made by blending these two slips, as follows:

B-1	100	Body "S"	0	Body "M"
B-2	90	"	10	"
B-3	80	"	20	"
B-4	70	"	30	"
B-5	60	"	40	"
B-6	50	"	50	"
B-7	40	"	60	"
B-8	30	"	70	"
B-9	20	"	80	"
B-10	10	"	90	"
B-11	0	"	100	"

Each body of the series was thoroughly mixed by blunging, and dried on concave plaster slabs down to stiff mud consistency, for the purpose of making into brickettes.

MAKING THE TRIAL PIECES.

For each of the bodies in Series A and Series B, brickettes were made 3½ x 1 x ½ inches. Two pin points were made on each brickette three inches apart for the purpose of measuring shrinkage, and the brickettes dried carefully so as to prevent warping.

BURNING THE TRIALS.

Brickettes from each body of the two series were burned at two different temperatures, cone 4 and cone 9. For the cone 4 burn cones 2, 3, 4 and 5 were placed on a slab at the center of a large sagger, and the brickettes placed around the cones in close proximity to them. The sagger was luted tight, placed at the center of the small updraft kiln, and fired

with coke as fuel for a period of eight hours, when cone 5, which was placed outside of the sagger, went down. It was observed in previous burns that there was a difference in the temperature between the outside and inside of the sagger, of approximately one cone.

On taking the trials from the kiln, it was found that cone 4 was down so that its point touched the slab, and cone 5 had began to curl.

The cone 9 burn was originally intended for cone 8. The trials were set in the same way as in the cone 4 burn, cones 6, 7, 8 and 9 being placed in the center. Coke was the fuel used, and the firing period lasted ten hours. Cone 9 was fired flat outside of the sagger. On taking the trials from the kiln, it was found that cone 8 inside the sagger was nearly flat, and cone 9 was down so that the point touched.

APPEARANCE OF TRIALS.

Series A in the cone 4 burn showed a gradual darkening in color from the kaolin-white in body A-1, down to a dark brownish gray in body A-11. The cone 9 burn gave a gradation in color from the white in A-1 to a stoneware gray in A-11. The colors at cone 9 were much lighter than at cone 4; in fact the brown had entirely disappeared.

Brickettes containing 50 per cent. or more of mica were highly vitrified in the cone 9 burn, and had the appearance of being thinly glazed. Such trial pieces had approached the first stages of fusion, as they were filled with minute bubbles and had swelled considerably.

No difference in general appearance could be detected between the two extremes of Series B, the entire series being a light ivory yellow in color for the cone 4 burn. In the cone 9 burn the yellow had disappeared, giving a biscuit which was nearly white.

DETERMINING FIRE SHRINKAGE AND POROSITY.

Before placing the trials in the kiln each brickette was carefully measured, and after firing, measurements were again taken. The loss in length due to fire shrinkage, divided by the dry length before firing, gave the percent of fire shrinkage.

Porosity was determined by absorption. Each brickette was weighed in the dry, burned condition on the balances, soaked in distilled water for forty-eight hours, removed, the superficial moisture wiped off and again weighed. The difference between wet and dry weights gave the grams of water absorbed by the pores. The weight of water absorbed, divided by the weight of the brickette before absorption, gave a ratio for the comparison of porosity. The following table gives the fire shrinkage and porosity for each body of the two series:

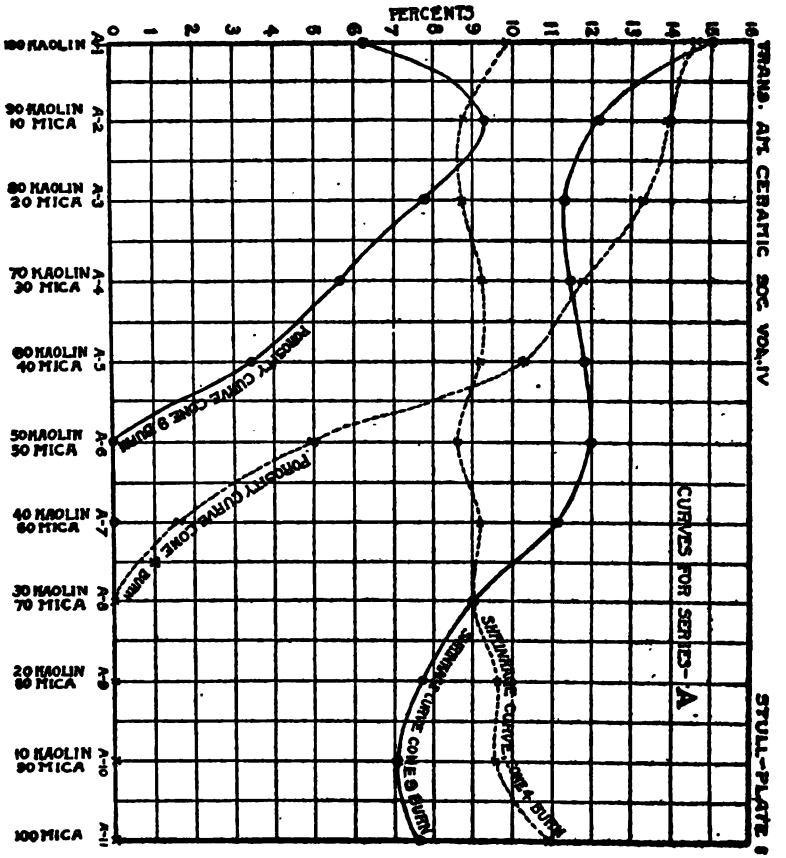
	Fire Shrinkage		Porosity	
	Cone 4	Cone 9	Cone 4	Cone 9
A- 1	9.87	15.13	14.58	6.84
A- 2	8.78	12.16	13.96	9.25
A- 3	8.78	11.41	13.40	7.88
A- 4	9.24	11.48	11.80	5.67
A- 5	9.21	11.84	10.24	3.43
A- 6	8.67	12.00	5.08	.00
A- 7	9.15	11.11	1.62	.00
A- 8	9.03	9.03	.05	.00
A- 9	9.68	7.80	.00	.00
A-10	9.61	7.01	.00	.00
A-11	10.90	7.60	.00	.00
B- 1	5.52	8.79	13.39	6.64
B- 2	4 97	7.69	14.13	6.81
B- 3	5.06	7.86	14.49	6.70
B- 4	4.47	7.18	15.23	7.74
B- 5	4.44	7 73	14.96	7.58
B- 6	3.87	7.14	16.13	7.97
B- 7	3.80	7.18	16.36	7.85
B- 8	3.91	7.22	16.70	8.22
B- 9	3.89	6.63	17.37	10.16
B-10	3.33	6.04	18.12	11.20
B-11	3.31	5.52	18.26	11.71

These results when plotted on curve sheets give some very interesting curves for study. On the following curve sheets the solid lines represent curves for the cone 9 burn, and the broken lines curves for the cone 4 burn.

STUDY OF THE CURVES.

In general, the shrinkage curve for the cone 4 burn shows a decrease in shrinkage from a content of 0 per cent. mica to 10 per cent. mica. From 10 per cent. to 70 per

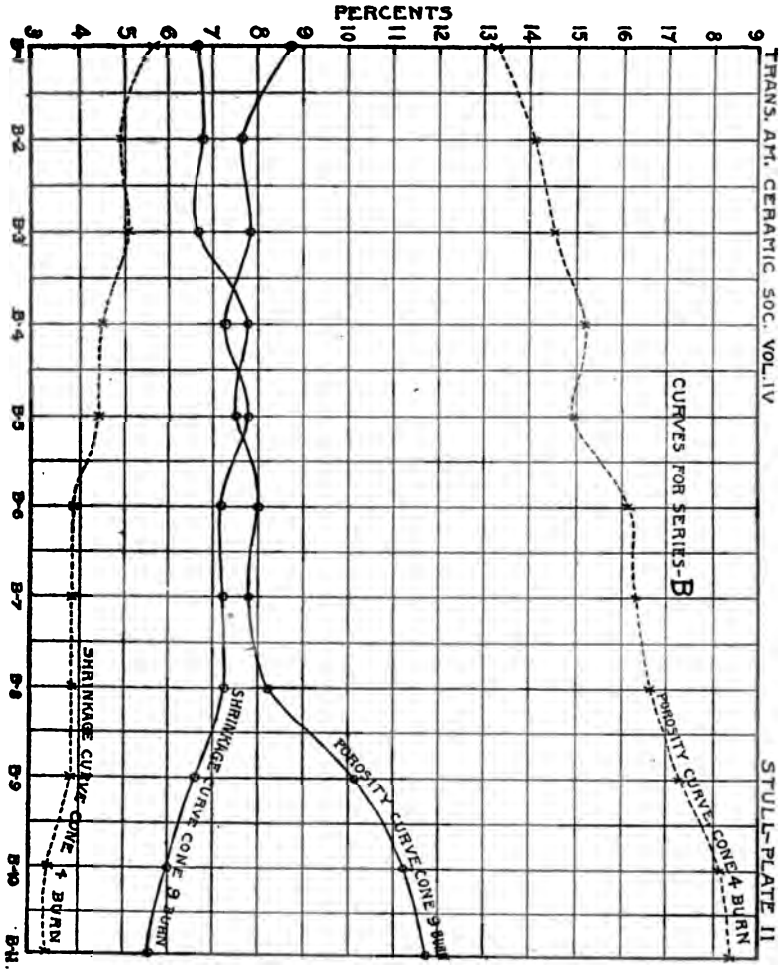
cent. mica the shrinkage is practically constant. From 70 per cent. to 100 per cent. mica there is an increase in shrinkage showing pure mica to have a shrinkage of 1 per cent. more than that of pure kaolin.



In the curve for the cone 9 burn there is a decrease in shrinkage from pure kaolin up to 20 per cent. mica. From 20 per cent. mica to 50 per cent. mica, the shrinkage increases. From 50 per cent. mica to pure mica the shrinkage drops.

Comparing the two shrinkage curves we see that they have a tendency to run parallel up to 50 per cent. mica. Beyond this point the tendeny is to run in opposite direc-

tions up to 90 per cent. mica. From 70 per cent. to 100 per cent. mica, the cone 4 burn has a greater shrinkage than the cone 9 burn. The difference in direction of the curve and in the decrease in shrinkage of the trials in the cone 9 burn, are accounted for by the swelling of the trial pieces when they contained from 50 per cent. to 100 per cent. mica. This was due to the vesicular structure produced in the advanced stage of vitrification or first stage of fusion.



The porosity curve for the cone 4 burn shows a continual decrease in porosity throughout, having no porosity

at 80 per cent. mica. In the cone 9 burn the curve shows an increase in porosity from 0 per cent. to 10 per cent. mica. From this point on porosity decreases, and when the content of mica reaches 50 per cent. porosity becomes zero. From a content of 10 per cent. mica, the two porosity curves run parallel.

With but one exception, the two porosity curves show that mica has exerted a fluxing action upon kaolin.

The one exception is a content of 10 per cent. mica in the cone 9 burn, where the porosity is greatest for that burn. This leads to the supposition that a content of 10 per cent. mica acts as a non-flux. This is also evidenced by the cone 4 and cone 9 shrinkage curves, which have their greatest falling off in shrinkage from pure kaolin to a content of 10 per cent. of mica.

The two shrinkage curves for Series B show a falling off in shrinkage from B–1 containing the feldspar to B–11 containing the mica. From B–6 to B–9 in the cone 4 burn, shrinkage remains constant. From B–6 to B–8 in the cone 9 burn, shrinkage also remains constant.

Both porosity curves for Series B show an increase in porosity from B.1 to B–11.

From the curves of Series B it is evident that mica has exerted a fluxing action, and that this fluxing action is less than that of feldspar.

SOURCES OF SHRINKAGE.

Fire shrinkage depends upon two factors, first, molecular contraction or condensation, and second, silicate formation produced by fluxing action. The latter shrinkage then depends upon the degree of vitrification brought about by the flux or fluxes present. When the flux has exerted no action, the body has had no secondary shrinkage—*i. e.*, this shrinkage is zero. When the flux has exerted its greatest power, then the body has its greatest shrinkage.

If the flux is present in sufficient quantities, the greatest shrinkage is at the point of the complete fusion. If the flux is present in small amounts, it may exert its full fluxing power long before fushion is reached, if no other flux is

present to continue the action. When the fluxing action has ceased, then the shrinkage depending upon it has ceased. Therefore, neglecting the shrinkage due to condensation the amount of which we do not know, it seems safe to say that the total shrinkage is roughly proportional to the fluxing power of the fluxes used. If we have two fluxes, which we will call C and D and place them under the same conditions and find that the body in which C was used gives a shrinkage of 5.52 per cent. and the body containing D gives 3.31 per cent. shrinkage, then

$$\frac{\text{Fluxing power C}}{\text{Fluxing power D}} = \frac{5.52}{3.31}$$

or fluxing power C is to fluxing power D=1 to 0.6. This is the case for Series B in the cone 4 burn. B–1 containing feldspar has a shrinkage of 5.52 per cent. and B–11 containing mica has 3.31 per cent. Since the difference in shrinkage due to molecular condensation is so small, we can take the above ratio as correct for all practical purposes.

B–1 contains 12.16 per cent. feldspar, and B–11 18.37 per cent. feldspar and B–11 18.37 per cent. mica by weight. Since the two bodies have the same general chemical constitution, we can say that the proportionate fluxing power of 12.16 pounds feldspar to 18.37 pounds mica=1 to 0.60 when placed under these conditions, and fired at the temperature of cone 4.

In the same way we find the proportionate fluxing power of 12.16 pounds feldspar to 18.37 pounds mica=1 to 0.63 when fired at the temperature of cone 9.

We see that these two proportions are practically the same. Multiplying the second and fourth members of the proportion by ten-sixths, gives the proportionate fluxing powers of 12.16 pounds spar to 30.61 pounds mica=1 to 1. Then 30.61 parts of mica are required in this case to produce the same fluxing action as 12.16 parts of feldspar, or 2.307 equivalents of mica produce the same fluxing power as one equivalent of feldspar, other things being equal.

CONCLUSIONS.

Drawing conclusions from the foregoing work, we have proven:

First. Mica, if fine enough, is plastic; hence, it need not necessarily cut down the plasticity of the clay.

Second. Mica, if fine enough, exerts a fluxing action upon kaolin and upon ceramic mixtures at temperatures below cone 4.

Third. Mica alone, when reduced to an extremely fine powder, vitrifies sufficiently to produce a non-absorbent body below the temperature of cone 4.

These results are contrary to the opinions expressed by the majority of ceramists. That mica is here proven to be a flux confirms the statement of Langenbeck and of Auscher et Quillard. Vogt, the eminent director of the National Porcelain Manufactory at Sevres, France, has shown that mica does not fuse at 1300 degrees C, but the condition under which the result was obtained is not known to the writer.

Mayer* and others have expressed their opinion in regard to the refractory qualities of mica by citing instances of finding mica flakes unchanged in the ware after passing the biscuit fire. These statements in regard to the refractory properties of mica, do not necessarily oppose the foregoing conclusions of this work. A substance to be an efficient and satisfactory body flux must be present in a fine state of division and disseminated uniformly throughout the mass. Substances present in coarse grains do not lose their identity, even when the mass is vitrified so as to be non-absorbent. Too much stress cannot be placed upon the advantages gained in fineness of grain. The finer the materials are prepared and the more intimately they are mixed, the nearer we get molecule to molecule, which materially aids both chemical action and fusion.

The mica used in this work was ground as fine as the feldspar, hence it had that same advantage of fusibility and chemical activity, in so far as fineness of the grain was concerned. Under these fair conditions we find that mica is unmistakably a flux, though not so good as feldspar.

*Vol. II, Trans. A. C. S., pp. 98, 99.

SOME GAPS IN OUR KNOWLEDGE OF CEMENTS.

(A NOTE.)

BY

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Notwithstanding the enormous development in the manufacture of cement, there yet remain many unanswered questions pertaining to its composition, its properties and peculiar reactions, some of which so far have baffled the most ingenious and refined attacks of research. It is the aim of this note to indicate a few of these problems to those who have not made a special study of this line.

Though apparently the composition of cements is a simple matter from the chemical standpoint, since it deals mainly only with the silicates and aluminates of lime and magnesia, we have at present no definite knowledge of the process of formation of these salts. We know in a general way that, with increasing temperatures, the basicity of the silicates and aluminates is increased, but exact information as to the proportions involved in these heat reactions is still lacking. Thus for instance, we are still at a loss to trace the connection between natural and Portland cements. This state of affairs is due to the fact that we are not yet in possession of data involving the examination of all the possible heat reactions with the help of accurate temperature measurements. It is absolutely essential to understand the fundamental principles involved in the formation of the simpler calcium compounds, before conclusions can be reached with reference to the complex combinations of Portland cement. If the elaborate experiments carried on by some of our able investigators were directed along this line, instead of dealing exclusively with such difficult problems as the determination of free lime in Portland cement, more rapid progress could perhaps be made. Synthesis certainly promises more direct results than analysis.

A series of synthetical compounds ranging, say, from 0.5 CaO (MgO) SiO_2 to 3.0 CaO (MgO) SiO_2, and from 0.5

$CaO\ (MgO)\ Al_2O_3$ to $2.0\ CaO\ (MgO)\ Al_2O_3$ burnt at different temperatures, exactly determined, is bound to produce results which will assist materially in clearing up the chemical laws governing the development of hydraulicity. In preparing these mixtures it is, of course, important that the materials be ground together as intimately as possible under exactly uniform conditions. Mere mixing is bound to be unsatisfactory. The progress of the combination with increasing temperatures can be followed by the following means:

1. Determination of the soluble silica (soluble in HCl and Na_2CO_3 solution) in the case of the lower lime compounds.

2. The heat of reaction of the resulting compounds as determined by the calorimeter.

3. The tensile strength of the cements as a measure of hydraulicity.

4. Determination of the water of hydration.

5. Boiling test.

6. Miscroscopic examination.

Perhaps the greatest confusion of opinion exists in regard to the relative hydraulic values of lime and magnesia and to illustrate the diversity of opinion existing on this topic, I shall quote somewhat at length as to various results obtained by a number of investigators.

In 1820, Fuchs, in working with several magnesia minerals such as talc, produced a good hydraulic cement. He found that magnesia burned at a lower temperature and was chemically more active than lime.

Heldt, another investigator, was not able to produce any hydraulic compounds from magnesia minerals and artificially prepared silicates.

Rivot again says, that magnesia behaves similarly to lime and forms with silica hydraulic compounds. He did not advise the use of magnesia cements, because the magnesia silicates and aluminates do not take up water as readily as the corresponding lime compounds.

Lieven, on the other hand, says that good cements cor-

responding to the formula $Mg\ SiO_3$ can be produced, which, when hydrated, possesses the formula $2(MgO\ SiO_2) .9\ H_2O$.

Beginning in the year 1887, a series of experiments was carried out by the Society of German Portland Cement Manufacturers, in which Dyckerhoff showed that magnesia in Portland cement, that is, in a vitrified cement promotes excessive expansion, and causes the cement to lose its property of being constant in volume. This result was vigorously disputed by A. Meyers and others, who claimed that the bad results were due to other deficiencies in the character of the cements tested.

In 1893, Kawalewsky made an extensive report on vitrified magnesia cements, which after ten years were still found to be constant in volume and which had shown high strength.

Golinelli, in 1895, reported a most carefully arranged series of tests with magnesium Portland cements, containing an average of 26 per cent. of MgO and corresponding to the ratio:

$$\frac{CaO + MgO}{SiO_2 + Al_2O_3 + Fe_2O_3} = 2.06.$$

The silica-alumina ratio maintained was 3. These cements possessed most excellent strength, and were entirely constant in volume. This investigator came to the conclusion that magnesia may well be substituted for the lime.

Prof. Tetmajer, perhaps the most widely known authority on the testing of cements, on examining two magnesia cements, found that they possessed a strength equal to that of Portland cement, and that they were constant in volume as tested by the standard boiling tests.

Spencer B. and W. B. Newberry, in 1897, arrived at the conclusion that magnesia, though possessing marked hydraulic properties when heated alone, yields no hydraulic products when heated with silica and alumina, and probably plays no part in the formation of cement. It is incapable of replacing lime in cement mixtures, the composition of which should be calculated on the basis of lime without regard to the magnesia present.

This great diversity of opinion can only be explained by the assumption that all these investigators worked under different conditions, neglecting the factors of temperature, mineralogical composition, fineness of grain, etc.

Evidence shows that many of our best American natural cements, such as the Rosendale, containing an average of 17 per cent. of magnesium carbonate, are equal in hydraulic value to those natural cements in which lime alone is present. Vitrified natural magnesia cements have already been tested some forty years ago by General Q. A. Gilmore, and were found to be highly hydraulic. Yet, some data show that if magnesia is present in a vitrified cement in a certain ratio to the lime, it is liable to cause trouble in hardening, owing to the fact that magnesia hydrates much more slowly than lime. The crystallization thus brought about by the magnesia falls into a period in which the lime silicates have already assumed a certain rigidity, thus giving rise to a tendency of the cement to increase in volume.

This seems to take place only under certain conditions, and hence many magnesia cements do not give any trouble at all. In the investigation of this subject, the difference existing between the natural magnesia cements burnt at a lower temperature, and magnesia cements burnt to vitrification should be duly considered, as this difference is both chemical and physical.

In the investigation of Portland cements, it is extremely important to study clays which, as we know, enter into the composition of many of our cements, and which often give rise to serious trouble in practical work. Cement chemists, as a rule, are perhaps inclined to study clays from the standpoint of the ultimate analysis almost exclusively without paying so much attention to their mineralogical character. We should remember that there are methods for the mineral examination of clays, as the method of Dr. Bischof, Professor Seger and the modified Lunge-Schochor method, adopted by the Ohio Geological Survey.

All clays may be said to be principally composed of three minerals: the clay substance proper, giving rise to plasticity and possessing the ideal composition Al_2O_3,

$2\,SiO_2$, $2\,H_2O$; feldspathic minerals remaining from incomplete decomposition of the original feldspar, and having the general formula RO, Al_2O_3 6, SiO_2; and quartz in various degrees of fineness.

These three minerals show marked differences in regard to their reactions with lime when subjected to heat. Clay substance, represented in its purest condition by washed kaolin or china clay, and in various degrees of impurity by the ordinary brick clays, reacts most readily with lime and is decomposed at very low temperatures, giving rise at first to a mono-silicate, which at a high temperature becomes a bi- and finally a poly-silicate. The first stage is the replacement of the combined water of the clay by the lime, a reaction which is most energetic, as is shown by the fact that clay substance decomposes calcium sulphate much more readily than silica at a lower temperature, and under but slightly reducing kiln conditions. After the expulsion of the chemical water, the union with more and more lime is a function of temperature.

The feldspathic minerals, though they do not combine with lime as energetically as clay substance, are not far behind in chemical activity, and owing to their high content of combined silica, they are an ideal basis for a cement. Beside introducing a desirable silica-alumina ratio, the minerals of the feldspathic type by means of their alkali content, lower the vitrification point of cements, thus decreasing the difficulty of burning.

The quantity of alkalies brought in, even if pure feldspar were used as the basis of cement, does not reach the point where it becomes injurious to the cement, for it must be remembered that combined alkali has an entirely different function from alkali brought in merely in the form of a carbonate. What Prof. Newberry says in regard to the addition of free alkali does not apply to a silicate like feldspar.

In this connection the interesting question of the production of white Portland cement might be mentioned. It seems to be an entirely feasible plan to produce a high grade white Portland cement by means of a white clay, high in

feldspathic matter, such as the Mt. Holly clay of Pennsylvania. The same end can be attained by preparing an artificial mixture of kaolin, feldspar and flint, which, of course, would be more expensive than the use of a naturally occuring easily fusible material. Several instances have come to the writer's notice, in which the production of a white cement was attempted simply by the use of kaolin and a lime bearing material. Since such a mixture, owing to the infusible nature of the kaolin, is extremely difficult to clinker, beside resulting in a highly aluminous cement, it is not surprising that the experiments proved failures.

The quartz unites with lime most reluctantly and only when finely ground, as has been shown by the writer in previous work, and hence the entire raw grinding process must be directed towards grinding the free quartz so fine that it will combine in the kiln with the desired amount of lime; but this has its commercial draw back. Experiments designed to show how far the grinding must be carried on in a tube mill, so as to produce a good cement from clays containing certain amounts of more or less coarse quartz are urgently needed, and would prove very valuable. Also, it would be exceedingly instructive to know just what size of quartz is absolutely necessary for combination with the lime in the rotary kiln. By knowing this, it might be possible to decrease the lime content, allowing a certain excess of inert silica, and preventing the presence of free lime. The problem certainly has a decided commercial interest.

This also brings us to the question of using quartz introduced into the cement mixture in the shape of sand stone. There is no reason why an addition of quartz to highly aluminous clays should not be feasible and practicable, though in this case it would be necessary to use a set of separate crushing and grinding machinery, which would grind the sand stone to quite a fine powder before it is added to the clay and lime materials. Since the amount of quartz thus required is comparatively small, it would seem that the expense of the extra grinding is not prohibitive.

Some interesting data could be collected by studying the residue insoluble in HCl and Na_2CO_3 solution of various

Portland cements, made from different raw materials; this residue, of course, consists practically of inert silica and carbon, and is bound to tell a tale of the sufficiency of grinding and combination.

Beside the mineralogical consideration of the clays used in cement making, the general chemical question of the best ratio between silica and alumina is one which deserves thorough attention, and would amply repay the labor of determining it. All who are acquainted with the manufacture of Portland cement, realize that clays too high in alumina produce cements apt to show very disagreeable and dangerous properties.

In the United States about three general types of grinding machinery are in use for this grinding of the raw and burnt cement. It is the object of grinding the raw mixture to establish as intimate a molecular contact as practicable, thus bringing about as complete a chemical reaction as possible. On the other hand, the clinker must be ground as finely as possible in order that the highest cementing power may be produced, and it is well known that the higher the percentage of the very finest particles in a cement, the greater will be the strength and bonding power. The amount of this extremely fine powder can be roughly estimated by the use of sieves, but this kind of test does not offer a sufficiently accurate basis of comparison. We can, however, arrive at a satisfactory determination by means of a mechanical separation of various sizes of particles, arbitrarily fixed. The most convenient method is that of suspension, in which the powder is allowed to settle a given length of time in beakers containing water if the raw mixture is to be examined, and 95 per cent. alcohol if a cement is to be subjected to the test. The sizes thus obtained are measured quite readily by means of a micrometer slide attached to a microscope. In this manner the true efficiency of a grinding machine can be determined, for the apparatus producing the greatest amount of the finest particles per unit of power is the most desirable machine.

Finally, the writer would like to allude to a most important economic problem, namely, the determination of the

suitability of the various coals for the burning of Portland cement in the rotary kiln, a question involving elaborate experiments and a great outlay, but amply justified by the importance of the matter at issue. The question which coals are and which are not suitable for this purpose, comprises besides the strictly thermal questions of calorific power, calorific intensity, inflammability, also the investigation of the effects produced by the sulphur in the coal.

In conclusion, it might be said that a number of the investigations outlined are now being carried on by the Ohio Geological Survey, and are being pushed towards completion as rapidly as possible. It is hoped by the Survey that the work may contribute some definite knowledge to the store of information already available, and that the American manufacturers may be induced to adopt the liberal policy, so splendidly manifested by the Society of German Portland Cement Manufacturers, to whom the world is indebted for so much scientific information. This body of manufacturers, by contributing large sums of money for the establishment of a fine laboratory, and the carrying on of research work for the benefit of its members, as well as by doing independent individual work, has accomplished results unexcelled in the annals of any manufacturer's organization. There is no reason whatever why the American Portland cement manufacturers should not accomplish as much or more, by simply co-operating along these lines. The benefits thus reaped would be of such magnitude, that the slight expenditure of time and money necessary would be positively insignificant.

NOTES ON THE DEVELOPMENT OF GREENS FROM CUPRIC OXIDE IN GLAZES.

BY

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The notes presented in this paper do not cover the subject as fully as I had intended, as I found my time too limited to undertake a larger investigation and have it ready for this meeting.

The experiments have been confined to the following basic oxides; lime, potash, soda, lead, barium, zinc and magnesia, displacing each other in the formulæ in regular order, so that the changes occuring, and the effects upon cupric oxide could be readily noted.

Green glazes produced with copper are subject to so many changes when under fire, that it becomes very necessary to use every care, both in the making and burning of the glaze. The least variation of the temperature or the method of firing the kiln, will change the color, varying from a bright green to a brown or yellow shade.

The volatility of copper also causes considerable trouble; the fumes are absorbed by the sagger, and are liberated when again placed in the kiln, tinting the ware in the sagger; a white glaze showing quite strongly the effect by absorption of the green color. To avoid difficulties of this kind it becomes necessary to have separate saggers for colors containing copper oxide. Where it is desired to produce transparent glazes of a green color at ordinary kiln temperatures, it is necessary to use oxide of copper; the other mineral oxides produce greens which are more or less opaque.

For the purpose of ascertaining the action of the basic oxides, the change of color and the fluxing produced in combination with oxide of copper, and a correction, if possible, of some of these troubles, a line of experiments was decided upon as follows:

First. The effect of the different basic oxides in combination, and their action in developing the green color from a constant content of cupric oxide.

Second. Increase of alumina content, keeping the silica content as a mono-silicate or bi-oxygen ratio.

Third. Increase of silica content, with the alumina content remaining constant.

Fourth. Replacement of silica content with an equivalent proportion of boric acid, retaining the same acidity with an increase in oxygen ratio.

Fifth. Increase of the alumina content with silica content remaining constant, thus producing a reduction of the acidity.

Sixth. Development under same conditions of fire and temperature on a silicious dust-pressed body, and an earthenware plastic body.

Seventh. Experiments to be made at cone 02 and 2, showing the change, if any, caused by the increased temperature.

To lessen the work as much as possible, at the same time to retain close enough interchange of the bases as outlined in the first proposition, 0.1 equivalent of cupric oxide was decided to be sufficient copper strength to show the changes, leaving 0.9 equivalent to make up the RO elements for all of the experiments.

This was done by making a change of .15 equivalent in each combination, beginning, as you will note, by referring to experiments one to six, at .45 equivalents each of two elements, down to .15 equivalent each of six elements, as seen in Nos. 91 to 97; being a gradual replacement and interchange of the basic oxides, and final elimination of lime in No. 97. The Al_2O_3 and SiO_2 content was the same in all of these experiments.

In the series of experiments Nos. 98 to 137, in which there is an increase of alumina, retaining the bi-oxygen ratio; increase of silica; introduction of boric acid; an increase of alumina, keeping constant content of silica; the RO elements were kept the same, to enable an easy and just comparison with each series.

The following table of formulæ gives a complete view of the glazes under consideration:

No.	CuO	CaO	K_2O	Na_2O	PbO	BaO	ZnO	MgO	Al_2O_3	SiO_2	B_2O_3
1	.10	.45	.45	....	...	....	..	....	.20	1.60	....
2	.10	.45	...	.45	...	....	....	....	.20	1.60	....
3	.10	.45	...	....	.45	....	....	....	.20	1.60	....
4	.10	.45	...	....	...	.45	.. .	....	.20	1.60	....
5	.10	.45	...	....	...	....	.45	....	.20	1.60	....
6	.10	.45	...	....	...	....	....	.45	.20	1.60	....
7	.10	.45	.30	.15	...	....	....	....	.20	1 60	....
8	.10	.45	.30	....	.15	....	....	...	.20	1.60	....
9	.10	.45	.30	....	...	.15	....	....	.20	1.60	....
10	.10	.45	.30	....	...	....	.15	....	.20	1.60	....
11	.10	.45	.30	....	...	....	....	.15	.20	1.60	....
12	.10	.45	.15	.30	...	....	....	....	.20	1.60	....
13	.10	.45	...	.30	.15	....	....	....	.20	1.60	....
14	.10	.45	...	.30	...	.15	....	....	.20	1.60	....
15	.10	.45	...	.30	...	....	.15	....	.20	1.60	....
16	.10	.45	...	.30	...	....	....	.15	.20	1.60	....
17	.10	.45	...	.15	.30	....	....	....	.20	1.60	....
18	.10	.45	.15	....	.30	...	....	....	.20	1.60	....
19	.10	.45	...	....	.30	.15	....	....	.20	1.60	....
20	.10	.45	...	....	.30	....	.15	....	.20	1.60	....
21	.10	.45	...	....	.30	...	....	.15	.20	1.60	....
22	.10	.45	...	....	.15	.30	....	....	.20	1.60	....
23	.10	.45	...	.15	...	.30	....	....	.20	1.60	....
24	.10	.45	.15	....	...	.30	....	....	.20	1.60	....
25	.10	.45	...	....	...	.30	.15	....	.20	1.60	. ..
26	.10	.45	...	....	...	.30	....	.15	.20	1.60	....
27	.10	.45	...	....	...	.15	.30	....	.20	1.60	....
28	.10	.45	...	....	.15	....	.30	....	.20	1.60	....
29	.10	.45	...	.15	...	....	.30	....	.20	1.60	....
30	.10	.45	.15	....	...	....	.30	....	.20	1.60	.....
31	.10	.45	...	....	...	....	.30	.15	.20	1.60	....
32	.10	.45	...	....	...	....	.15	.30	.20	1.60	....
33	.10	.45	...	....	...	.15	....	.30	.20	1.60	....
34	.10	.45	...	....	.15	....	....	.30	.20	1.60	....
35	.10	.45	...	.15	...	....	....	.30	.20	1.60	....
36	.10	.45	.15	....	...	....	....	.30	.20	1.60	....
37	.10	.30	.30	.30	...	....	....	....	.20	1.60	....
38	.10	.30	.30	....	.30	....	....	....	.20	1.60	....
39	.10	.30	.30	....	...	.30	.. .	....	.20	1.60	....
40	.10	.30	.30	....	...	....	.30	....	.20	1.60	....
41	.10	.30	.30	....	...	....	....	.30	.20	1.60	....

No.	CuO	CaO	K_2O	Na_2O	PbO	BaO	ZnO	MgO	Al_2O_3	SiO_2	B_2O_3
42	.10	.30	...	.30	.30	....	....	....	.20	1.60	....
43	.10	.30	...	.30	...	.30	....	....	.20	1.60	....
44	.10	.30	...	.30	...	....	.30	....	.20	1.60	...
45	.10	.30	...	.30	...	....	...	.30	.20	1.60	....
46	.10	.30	...	....	.30	.30	....	...	.20	1.60	....
47	.10	.30	...	....	.30	....	.30	....	.20	1.60	....
48	.10	.30	...	...	.30	....	....	.30	.20	1.60	....
49	.10	.30	...	....	...	.30	.30	....	.20	1.60	....
50	.10	.30	...	....	...	.30	....	.30	.20	1.60	....
51	.10	.30	...	....	...	....	.30	.30	.20	1.60	....
52	.10	.30	.30	.15	.15	....	...	....	.20	1.60	...
53	.10	.30	.30	.15	...	.15	....	....	.20	1.60	....
54	.10	.30	.30	.15	...	....	.15	....	.20	1.60	...
55	.10	.30	.30	.15	...	....	....	.15	.20	1.60	....
56	.10	.30	.30	....	.15	.15	....	....	.20	1.60	....
57	.10	.30	.30	....	.15	....	.15	....	.20	1.60	....
58	.10	.30	.30	....	.15	....	....	.15	.20	1.60	....
59	.10	.30	.30	....	...	.15	.15	....	.20	1.60	....
60	.10	.30	.30	....	...	.15	....	.15	.20	1.60	....
61	.10	.30	.30	....	...	....	.15	.15	.20	1.60	....
62	.10	.30	...	.30	.15	.15	....	....	.20	1.60	....
63	.10	.30	...	.30	.15	....	.15	....	.20	1.60	....
64	.10	.30	...	.30	.15	....	....	.15	.20	1.60	...
65	.10	.30	.15	.30	.15	....	....	....	.20	1.60	....
66	.10	.30	...	.30	...	.15	.15	....	.20	1.60	....
67	.10	.30	...	.30	...	.15	....	.15	.20	1.60	....
68	.10	.30	.15	.30	...	.15	....	....	.20	1.60	...
69	.10	.30	...	.30	...	....	.15	.15	.20	1.60	....
70	.10	.30	.15	.30	...	....	.15	....	.20	1.60	....
71	.10	.30	.15	.30	...	....	....	.15	.20	1.60	....
72	.10	.30	...	....	.30	.15	.15	....	.20	1.60	....
73	.10	.30	...	....	.30	.15	....	.15	.20	1.60	....
74	.10	.30	...	.15	.30	.15	....	....	.20	1.60	....
75	.10	.30	.15	....	.30	.15	....	....	.26	1.60	....

No.	CuO	CaO	K_2O	Na_2O	PbO	BaO	ZnO	MgO	Al_2O_3	SiO_2	B_2O_3
76	.10	.30	...	....	.30	....	.15	.15	.20	1.60	....
77	.10	.30	...	.15	.30	....	.15	....	.20	1.60	....
78	.10	.30	.15	....	.30	....	.15	....	.20	1.60	....
78-A	.10	.30	...	.15	.30	.	....	.15	.20	1.60	....
78-B	.10	.30	.15	....	.30	....	...	.15	.20	1.60	....
79	.10	.30	...	....	...	.30	.15	.15	.20	1.60	....
80	.10	.30	...	....	.15	.30	.15	....	.20	1.60	....
81	.10	.30	...	.15	...	.30	.15	....	.20	1.60	....
82	.10	.30	.15	....	...	.30	.15	....	.20	1.60	....
82-A	.10	.30	...	....	.15	.30	....	.15	.20	1.60	....
82-B	.10	.30	...	.15	...	.30	....	.15	.20	1.60	....
82-C	.10	.30	.15	....	...	.30	....	.15	.20	1.60	....
83	.10	.30	...	....	...	.15	.30	.15	.20	1.60	....
84	.10	.30	...	....	.15	....	.30	.15	.20	1.60	....
85	.10	.30	...	.15	...	....	.30	.15	.20	1.60	....
86	.10	.30	.15	....	...	....	.30	.15	.20	1.60	....
87	.10	.30	...	...	...	.15	.15	.30	.20	1.60	....
88	.10	.30	...	....	.15	....	.15	.30	.20	1.60	....
89	.10	.30	...	.15	...	....	.15	.30	.20	1.60	....
90	.10	.30	.15	....	...	....	.15	.30	.20	1.60	....
91	.10	.15	...	.15	.15	.15	.15	.15	.20	1.60	....
92	.10	.15	.15	....	.15	.15	.15	.15	.20	1.60	....
93	.10	.15	.15	.15	...	.15	.15	.15	.20	1.60	....
94	.10	.15	.15	.15	.15	....	.15	.15	.20	1.60	....
95	.10	.15	.15	.15	.15	.15	....	.15	.20	1.60	....
96	.10	.15	.15	.15	.15	.15	.15	....	.20	1.60	....
97	.10	...	.15	.15	.15	.15	.15	.15	.20	1.60	....
98	.10	.30	.15	....	.30	....	.15	....	...	1.00	....
99	.10	.30	.15	....	.30	....	.15	....	.05	1.15	....
100	.10	.30	.15	....	.30	....	.15	....	.10	1.30	....
101	.10	.30	.15	....	.30	....	.15	....	.15	1.45	....
102	.10	.30	.15	...	.30	...	.15	....	.20	1.60	....
103	.10	.30	.15	....	.30	....	.15	....	.25	1.75	....
104	.10	.30	.15	....	.30	....	.15	....	.30	1.90	....
105	.10	.30	.15	....	.30	....	.15	....	.35	2.05	....
106	.10	.30	.15	...	.30	....	.15	....	.40	2.20	....
107	.10	.30	.15	....	.30	....	.15	....	.20	1.00	....
108	.10	.30	.15	....	.30	....	.15	....	.20	1.25	....
109	.10	.30	.15	....	.30	....	.15	....	.20	1.50	....
110	.10	.30	.15	....	.30	....	.15	....	.20	1.75	....
111	.10	.30	.15	....	.30	....	.15	....	.20	2.00	....
112	.10	.30	.15	....	.30	....	.15	....	.20	2.25	....

No.	CuO	CaO	K_2O	Na_2O	PbO	BaO	ZnO	MgO	Al_2O_3	SiO_2	B_2O_3
113	.10	.30	.15	....	.30	....	.15	....	.20	2.50	....
114	.10	.30	.15	....	.30	....	.15	....	.20	2.75	....
115	.10	.30	.15	....	.30	....	.15	....	.20	3.00	....
116	.10	.30	.15	....	.30	....	.15	....	.20	3.25	....
117	.10	.30	.15	....	.30	....	.15	....	.20	3.50	....
118	.10	.30	.15	....	.30	....	.15	....	.20	1.75	....
119	.10	.30	.15	....	.30	....	.15	....	.20	1.70	.05
120	.10	.30	.15	....	.30	....	.15	....	.20	1.65	.10
121	.10	.30	.15	....	.30	....	.15	....	.20	1.60	.15
122	.10	.30	.15	....	.30	....	.15	....	.20	1.55	.20
123	.10	.30	.15	....	.30	....	.15	....	.20	1.50	.25
124	.10	.30	.15	....	.30	....	.15	....	.20	1.45	.30
125	.10	.30	.15	....	.30	....	.15	....	.20	1.40	.35
126	.10	.30	.15	....	.30		.15	....	.20	1.35	.40
127	.10	.30	.15	....	.30	....	.15	....	.20	1.30	.45
128	.10	.30	.15	....	.30	....	.15	....	.20	1.25	.50
129	.10	.30	.15	...	.30	....	.15	....	.10	1.60	....
130	.10	.30	.15	....	.30	....	.15	....	.15	1.60	....
131	.10	.30	.15	...	.30	....	.15	....	.20	1.60	....
132	.10	.30	.15	....	.30	....	.15	....	.25	1.60	....
133	.10	.30	.15	....	.30	....	.15	....	.30	1.60	....
134	.10	.30	.15	....	.30	....	.15	....	.35	1.60	....
135	.10	.30	.15	....	.30	....	.15	....	.40	1.60	....
136	.10	30	.15	....	.30	....	.15	....	.45	1.60	....
137	.10	.30	.15	....	.30	....	.15	....	.50	1.60	....

In these experiments the K_2O was obtained from pure carbonate potash, Na_2O from crystal carbonate of soda, PbO and BaO from the carbonates and ZnO and MgO from the oxides. Al_2O_3 was obtained entirely from clay, .05 equivalent being raw clay and the balance calcined clay.

The carbonates of potash, soda and barium, and boric acid were fritted, while the other elements were used without fritting, as was the oxide of copper.

The glazes were all carefully prepared and passed through a brass lawn of 200 meshes to the inch. The tiles were glazed and placed together in the same kiln and fired at cone 02. A second lot was placed in an other kiln and fired at cone 2.

THE RESULTS.

The benefit of these experiments can only be gained by a careful examination of the samples. No words can convey the numberless delicate shading of the tints produced. Nevertheless, the pen can describe a few of the coarser or more obvious distinctions, which are given below:

In glazes 1 to 6, the RO is composed of .45 equivalent of CaO and .45 equivalent of one of the other elements. this being the highest quantity of any of the basic oxides thought advisable in this line of experiments.

No. 3, containing PbO, was the only one showing satisfactory glaze results; it developed a very dark green, both at cone 02 and 2, the former having considerable scum over the surface, with well defined crystals around the edges, while the latter was a very clear glaze with a few small crystalline spots on the surface.

The other glazes in this series were immature at both cone 02 and 2, each base, however, showing different action in developing the copper, as well as fusibility. K_2O gave a yellowish green, while the Na_2O leaned to the gray; BaO developed in its semi-vitreous state at cone 02 a turquois green, but lost the color at the higher heat, changing to a blackish green; the ZnO at the lower heat had a chrome green shade, changing at cone 2 to a darker color. Their fusibility was in the order named: PbO, Na_2O, ZnO, K_2O and BaO; the MgO was infusible, the CuO having passed

into the body, leaving the glaze without any color and peeled from the body.

The details of this series has been given more fully than will be possible with the others, to show the action of the different basic oxides when in combination with CaO, and comparison when combined with two or more of the basic oxides.

In glazes 7 to 11 where the K_2O has been reduced to .30 equivalents, and combined with .15 equivalents of one of the other bases, the introduction of the third base gives an entirely different color effect. The zinc glaze, No. 10, is the only one showing a fully developed glaze at cone 2. It had a light bluish iridescence, while at cone 02 it was more of a chrome shade with soft velvety matt effect.

BaO, No. 9, gives a grayish green matt glaze at cone 2, while PbO, No. 8, is a dark blackish green color, iridescent crystaline surface with apparently a clear glaze underneath at cone 2. MgO, No. 11, shows some fusibility at cone 2, with good color effect.

In the glazes 12 to 16, where the .30 equivalents K_2O has been replaced by Na_2O, all show more fusibility than the former series. ZnO, No. 15, is, however, the only clear glaze at cone 2, as it was in the previous series, and a light sea green shade; at cone 02 a dark brown matt glaze was developed.

By the introduction of .30 equivalents of PbO in glazes 17 to 21, a line of more fusible glazes are obtained, with entirely different results from the preceding experiments. The ZnO member—No. 20—is a beautiful clear green, the K_2O shows most fusibility, and the Na_2O has many small crystals on the surface.

The BaO glaze, No. 19, is covered with a scum, showing some tendency to crystalize and is the only one in this series that shows any crazing. At cone 02 this series is thoroughly fused, but the heat is not sufficient to fully develop the glazes.

There is very little variation in the color effect in this series; the increased content of PbO seems to negative the

action of the other bases on copper which is very noticeable in comparing No. 18 with No. 8, or No. 17 with No. 13.

BaO in No. 22 to No. 26, does not give the fusible qualities expected from it, as none of the glazes have cleared. K_2O in No. 24 seems to excite the fluxing qualities of BaO more than the other bases.

In the series containing .30 equivalents ZnO, No. 27 to No. 31, the one with BaO, No. 27, is more fusible than the barium zinc combination, No. 25, which contains a larger quantity BaO. The increase of ZnO excites the fluxing, while the reverse is the case with PbO, No. 28, when compared with No. 20. The most fusible of this series is the Na_2O member, No. 20, which gives a very dark clear grass green, deepening and changing the color when compared with No. 15 which is much lighter and more of a turquoise, while the one with K_2O, No. 30, has a bluish cast more of a sea green, with some fine crystals on the surface.

Nos. 27 and 28 at cone 02 are matt glazes, the former showing some signs of crystalizing, while the latter produces a very soft velvety matt surface of a bronze green color. No. 29 at cone 02 gives a semi-matt glaze, of a chrome-green color.

In the glazes No. 31 to No. 36, containing .30 equivalents MgO, there is no signs of fusion at cone 02, the glaze cracking and peeling from the body, while at cone 2, No. 34, with PbO, is vitrified with dull surface, No. 36 and No. 35 being next in order of fusibility.

The order of fusibility with .15 equivalent of MgO is No. 21, No. 16, No. 11, No. 26, and No. 31, containing .45 Eq. CaO, and .30 Eq. PbO, Na_2O, K_2O, BaO, and ZnO respectively.

In all of the glazes from No. 1 to 36, containing .45 equivalent CaO, there are none of practical use at cone 02, but several at cone 2; No. 20 being a very good glaze, dark green with slight yellow cast, while at cone 02 there are several excellent dull or matt glazes, as above mentioned.

In considering glazes 37 to 41, you will note the first reduction in CaO content to .30 equivalent, dividing the other bases into .30 equivalent each.

A reduction of CaO and the addition of .15 equivalent of one of the other bases, when compared with series No. 7 to No. 11 shows increased fusibility, except in No. 41, MgO; No. 40 containing ZnO gives a bluish opaque color at cone 2 while at cone 02 a semi-mat blue-green glaze. In No. 39 we have a peculiar black-green matt surface at both cone 2 and 02, with very fine crystals covering the surface at cone 2.

In the soda Series, No. 42 to No. 45, there is an increased fusibility when compared with No. 12 to No. 16, with the exception of No. 45, MgO. The whole series is more fusible than the one containing K_2O. The color in No. 44 at cone 02 gives a gray-green matt surface and loses the blue color produced by K_2O in No. 40.

In Nos. 46 to 51, which closes the .30 equivalent series, are some interesting effects. In No. 47 the increase of zinc does not perceptibly affect the color or fusibility in comparison with No. 20, except that in the former, small floculent spots appear suspended in the glaze. No. 49 has qualities worthy of further investigation, producing a semi-glossy surface of very good dark green. Nos. 48 to 51 are matt surfaces, the first a yellow-green, while the other is a gray-green.

In the next series of experiments, a fourth element is introduced in the RO, with increased fusibility and change of color all through the series; but I will not attempt to give a detailed analysis, only calling your attention to a few of the most marked changes. Again, the larger content of Na_2O over K_2O shows in a marked degree more fusibility and decided difference in color effect, as seen by comparing No. 53 and No. 54 with No. 68 and No. 70 at cone 2.

Nos. 58, 59, 61, 69 and 78B at cone 2 are dull on semi-dull surfaces, the colors being entirely different, varying from a gray to a blue and brownish-green. The surface of No. 69 is covered with fine gray needle crystals, with a darker green in the body glaze, and is worthy of further consideration as are all the others.

The Series No. 70 to No. 82, containing .30 equivalents

of barium produces at cone 2, some dull effects that can be followed to advantage in some further experiments.

No. 85 is a fully developed, very dark brilliant green glaze at cone 2, while at cone 02 shows little signs of fusion.

The first combination with MgO, giving satisfactory results, is the one with lead and zinc, No. 76, giving a dark, clear green glaze, but practically the same color as the ones with K_2O and Na_2O content, No. 77 and No. 78, indicating that MgO has no peculiar action in producing copper greens.

In the series containing an equal division of the RO elements, the results were not as anticipated, as more fusible glazes were expected. The only ones showing any tendency to a clear glaze, were Nos. 91, 92, 94 and 96, at cone 2, No. 94 being a fully developed dark green glaze. No. 97, the only one in the entire series without CaO, gives every indication of a good glaze at a little higher heat, and does not indicate that lime has any decided effect in developing copper greens.

In Nos. 98 to 106 is an increase of Al_2O_3 with corresponding increase of SiO_2, keeping the bi-oxygen ratio, beginning at a very low SiO_2 content without any Al_2O_3 and increasing by .05 equivalent up to .40 equivalent, and in Nos. 129 to 137 is an increase of Al_2O_3, without any change in SiO_2 content, reducing from an oxygen ratio of 2.46 to 1.28.

The effect of an increase of Al_2O_3 is very marked, the color gradually changing from a bright emerald green to a very deep green in No. 101 and then gradually getting lighter to a yellow green with fairly satisfactory glazes at cone 2 up to the one containing .30 equivalent of Al_2O_3, No. 105, and at cone 02, up to .20 equivalent Al_2O_3, No. 102, with very slight variation of color between the two temperatures on the dust pressed body, while on the earthenware body at cone 02, the glazes are better developed, but show a tendency to crazing.

No. 98 without any alumina, is a very good glaze at cone 2, while at cone 02, small crystals are seen along the

edges and on the surface of the glaze; No. 106 begins to show a flaky crystalline effect from the increase of Al_2O_3.

The same effect of producing a yellow green is also very noticeable in Series Nos. 129 to 137, giving a dark green up to No. 131, when the yellow begins to show its effect on the color, by the increase of Al_2O_3, but not so much as in the other series; .10 equivalent at cone 02 and .25 equivalent at cone 2 seems to be the limit of Al_2O_3 in producing glazes with SiO_2 content of 1.60 equivalent. The glazes beyond these qualities having a matt effect with a soft velvety surface on the higher Al_2O_3 content at cone 02 and cone 2; .40 equivalent produces the best mat surface at cone 02 and .50 equivalent at cone 2.

This line of experiments on the earthenware body are more fusible, without any change in color.

With the increase of silica, maintaining alumina constant, Series Nos. 107 to 117, we have good glazes from Nos. 108 to 113 at cone 2, and fairly good at cone 02, the latter showing some tendency to creep, which no doubt can be corrected by a mechanical change in the mixing of the glaze. The higher silica glazes of this series are in various stages of devitrification, the first indication appearing in No. 111, but all of them produce a rough texture.

There is a decided difference between Nos. 107 and 108, .25 equivalent of SiO_2 changing the glaze from a matt to practically a clear glaze at cone 02, and at cone 2 to a beautiful shade of green with a little tendency to crazing. No. 107 at cone 2 has lost the matt, changing into a fully vitrified glaze, covered with small crystals. The texture of the matt effect in No. 107 is very much the same as produced by high Al_2O_3 content, and compares very closely to No. 133 in surface, but not in color. Increased SiO_2 content has no perceptable change in color at either temperature.

The earthenware body in this series gives about the same degree of fusion at cone 02 as the dust pressed body does at cone 2, but no difference in the color.

The boric acid series, Nos. 119 to 128, gives a good line of glazes at cone 2 with very little change in color, but showing some signs of bleaching to a lighter color; but at

cone 02 on the dust pressed body, the lower series of B_2O_3 are somewhat crystalized and looks very much like the beginning of divitrification, decreasing as the B_2O_3 increases; Nos. 122 to 128 are clear glazes, the higher content giving a brighter green. On the earthenware body, the glazes are all good, but there is considerable change in color between cones 02 and 2, the latter giving an entirely different quality of green.

The separating and creeping of many of these glazes, can be corrected by changing the mechanical mixing, either in the glaze or the making of the fritt, but in this line of experiments a uniform method was used to get exact comparison of the peculiar action of the RO elements when in combination with oxide copper.

The deductions to be derived from this line of experiments are such as to excite the desire for a further investigation, and makes me hesitate to place before you what I consider is incomplete; and in doing so, I sincerely hope my results will be the means of starting some one on a similar and more complete analysis of the development of greens from cupric oxide. I have not had the time to verify any of these experiments, and give them to you merely as first results.

The alumina content of .20 equivalent in Nos. 1 to 97 was decided upon, after several preliminary trials, as the best content for this investigation. A different proportion might have changed some of the results, producing more satisfactory glazes, but without giving the same comparison.

From these experiments, the indications are that CaO can be used in any quantity found necessary in producing a satisfactory glaze, without affecting the color produced from copper. But with the introduction of K_2O, the color is changed to a yellowish tint; and and when in combination with either Na_2O or PbO, the K_2O can not exceed .15 equivalent without affecting the color; but ZnO controls the green color when content of K_2O is as high as .30 equivalent: the color becomes very much deeper as the K_2O decreases and the ZnO is increased. This same effect of

deepening the color is produced when ZnO is combined with Na_2O, but with an entire change of color.

The same can be said of the zinc-barium glaze, the color getting stronger when the ZnO is increased to .30 equivalents and BaO decreased to .15 equivalents. BaO is more fusible when in combination with ZnO than with Na_2O, producing a better working glaze. The barium-zinc glaze has decidedly a blue shade when combined with either Na_2O or K_2O, but with PbO has more of a chrome green color. BaO when with .15 equivalents of MgO produces a color very different from that produced with PbO.

ZnO when in combination with either K_2O or Na_2O and not exceeding .15 equivalents gives a light clear glaze leaning to the blue; but when the ZnO is increased to .30 equivalents, and the CaO reduced to .30 equivalents, the one with K_2O produces a lighter shade, decidedly blue and opaque; while the one with Na_2O changes color more to the brown, but gives a clear glaze.

ZnO has a very decided action in producing colors in both clear and matt glazes; .15 equivalent shows a more marked effect than .15 of any of the other bases. ZnO can be used to advantage with all basic oxides, making the color more brilliant when in combination with PbO, and giving most beautiful and varied shades from copper when in other combinations. I am not prepared to say conclusively that ZnO has a strong tendency to excite the fluxing with cupric oxide, but every indication points to such action.

.30 equivalent of PbO when in combination with .45 equivalents of CaO and .15 equivalents of one of the other bases, controls completely the color, producing a clear deep green; but loses this effect when reduced to .15 equivalents: with the reduction of the CaO to .30 equivalents, the PbO does not have the same power, the color being modified according to the other base introduced.

Replacement of CaO with MgO retards fusion, without changing the color, except in the one with PbO, which is a very much lighter shade of green; and more of a gray-green when the MgO is increased.

With the introduction of boric acid, replacing equiva-

lent contents of silica, we have a line of colors showing but slight changes, the largest B_2O_3 content being a little lighter but more brilliant green; while the SiO_2 content in all proportions seem to give about the same shade; but with alumina the content must be kept down to secure a pure green color.

In summing up the facts demonstrated from these experiments, SiO_2 and B_2O_3 can be used in any desired content, while Al_2O_3 should not exceed .25 equivalents, unless the shade of green desired is to be of yellow tint; while in the RO elements, not over .30 equivalents should be used of any one base for best effects.

I have endeavored to deduce something from these experiments that will prove interesting and of advantage, but fully realize my lack of descriptive power to convey intelligently to you the color changes. I am fully convinced there is but on way for you to get any real benefit from this paper, and that is for you to make a similar line of experiments, and I certainly cannot urge you too strongly to do so, assuring you that the labor connected therewith will be fully repaid by the interesting study you will have when completed.

NOTE BY THE EDITOR:—The effort has been made in the following pages to reproduce the color series which Mr. Walker exhibited while reading his paper. The effort has been only partly successful, as it only shows the color effects by variations of shade, and cannot bring out the marvelous differences of quality of color. However, the plates in connection with the descriptive text will help the careful student to gain some impressions which would otherwise be wholly lost.

1
2
3
4

Series Fired at Cone 2.

---

1. *Composition.* 0.10 CuO 0.45 CaO 0.45 K_2O, 0.20 Al_2O_3 1.60 SiO_2.

An imperfectly fused, matt glaze, with signs of glossy surface around edges. Not fluid enough to heal over the defects formed during shrinkage. Color, dirty yellowish-green around edges, which show dark in the cut. In the center, the color shades into a steel gray, which reflects light and appears lighter in the cut. No promise for any practical purpose.

---

2. *Composition.* 0.10 CuO 0.45 CaO 0.45 Na_2O, 0.20 Al_2O_3 1.60 SiO_2.

A much better fused glaze, smooth to the touch. It is not a good matt, but is equally removed from a glossy surface. Shows few lines, indicating where shrinkage cracks formed and subsesequently mended. Color, apple-green around edges, which show dark in cut, but shading into a dark grayish-black color in center, which appears lighter in the cut Not attractive.

---

3. *Composition.* 0.10 CuO 0.45 CaO 0.45 PbO, 0.20 Al_2O_3 1.60 SiO_2.

A brilliant perfectly fused glaze of high finish. A few smal blotches of some undissolved matter, seen faintly in the upper left-hand corner of the cut. Color, a brilliant clear copper-green, apparently equally removed from the blue or yellow tints. Contrast is due to the difference in depth of glaze, which makes the color very dense in left-hand upper corner. Could be used, as far as color or surface is concerned, for any purpose.

---

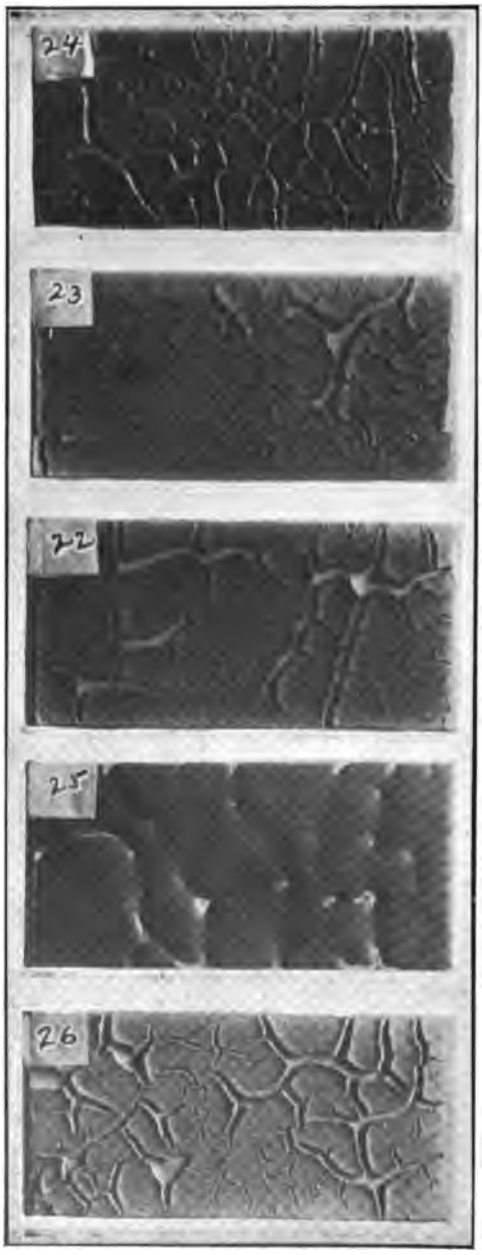
4. *Composition.* 0.10 CuO 0.45 CaO 0.45 ZuO, 020 Al_2O_3 1.60 SiO_2

A semi-crystalline matt, whose imperfectly obliterated shrinkage lines form a rather coarse net work, giving a peculiar and rather pleasing curdy appearance. Surface feels much less smooth than No. 2 and smoother than No. 1. Color is an opaque blue-green, almost of the tint known as "robin's egg" blue. A glaze which invites further development.

---

5. *Composition.* 0.10 CuO 0.45 CaO 0.45 ZuO, 0.20 Al_2O_3 1.60 SiO_2.

An imperfectly fused glaze, which has not flowed enough to cover all of its shrinkage cracks, though it has covered some. Surface is matt, mostly, but in a few patches, especially on left hand end, the scum breaks away and exposes a glossy surface. The scum or matt surface gives this tile a grayish-green cast, which appears clear and darker in color where the film is broken away. Not a workable glaze at present.


24
23
22
25
26

Series Fired at Cone 02.

0.20 Al_2O_3, 1.60 SiO_2 throughout series.

---

24. *Composition.* 0 10 CuO 0.45 CaO 0 30 BaO, 0.15 K_2O.

A crackled, shrunken mass, just beginning to soften and round off the edges and corners. Color, light bluish-green around edges of tile, but a strong dark chrome green in all the central portion of tile.

---

23. *Composition.* 0.10 CuO 0.45 CaO 0.30 BaO, 0.15 Na_2O.

A much dryer, less vitreous mass than 24, cracks numerous and edges sharp. Would not have fused for a number of cones higher. Color faded out to a light blue around edges. In central portion, the color is a dark neutral tint, or slate blue, with a slightly purplish cast. No likelihood of this glaze becoming useful

---

22. *Composition.* 0.10 CuO 0.45 CaO 0 30 PaO, 0.15 PbO.

A still less fused mass, shrunken and vitrified and crossed by numerous deep, sharp edged cracks. Volatilization has bleached edges to a clear light blue-green, but center is a much darker shade of the same color, not unlike No. 23, but bluer. No utility. This slab is remarkable, in view of the high fluxing value of lead and its small effect here, compared to fluxes which are elsewhere less active.

---

25. *Composition.* 0.10 CuO 0.45 CaO 0.30 BaO, 0.15 ZnO.

A smooth, satin-finish mass, imperfectly fluid, not wholly obliterating the shrinkage cracks, through which the body sometimes shows. Texture wonderfully fine. Color, a beautiful dark chrome-green enamel, marked with black lines where cracks have been healed over. If a little more fluid, so as to cover the body wholly, this would make a fascinating surface finish for art ware.

---

26 *Composition.* 0.10 CuO 0.45 CaO 0.30 BaO, 0.15 MgO.

A dry, imperfectly vitrified substance, shrunken and cracked through to the body, and with edges sharp and rolled upwards somewhat. Color, light blue-green or robin's-egg blue. The absence of green in this color makes the cut show light in tone. No signs of probable utility.

37
38
39
40
41

Series Fired at Cone 2.

0.20 Al_2O_2 1.60 SiO_2 throughout Series.

---

37. *Composition.* 0.10 CuO 0 30 CaO 0.30 K_2O 0.30 Na_2O.

Fairly well fused, obliterating all shrinkage cracks. Covered with a whitish crystalline scum, which destroys the color of the glaze and makes it appear gray in cut. Around edges, the color is an opaque green with a somewhat bluish tint. No utility.

---

38. *Composition.* 1.10 CuO 0.30 CaO 0.36 K_2O 0.30 PbO.

A brilliant glossy surface, disfigured by crystalline segregations on right hand lower corner. Oily spots or drops in the glaze, give it a dappled effect by reflected light. This does not show in the cut. Color, a pure dark green, like No. 3. Difference in tint in cut due to drainage and thicker glaze at those points. This glaze is feasible for use, but not as good as No. 3.

---

39. *Composition.* 0.10 CuO 0.30 CaO 0.30 K_2O 0.30 BaO.

A shrunken, crackled mass, in which the edges have fused and rounded and the cracks healed over in part. Color is rather dull dark bluish-green, not dissimilar to No. 4, which is similar in composition. Glaze not valuable in its present shape, but offers a good lead for investigation.

---

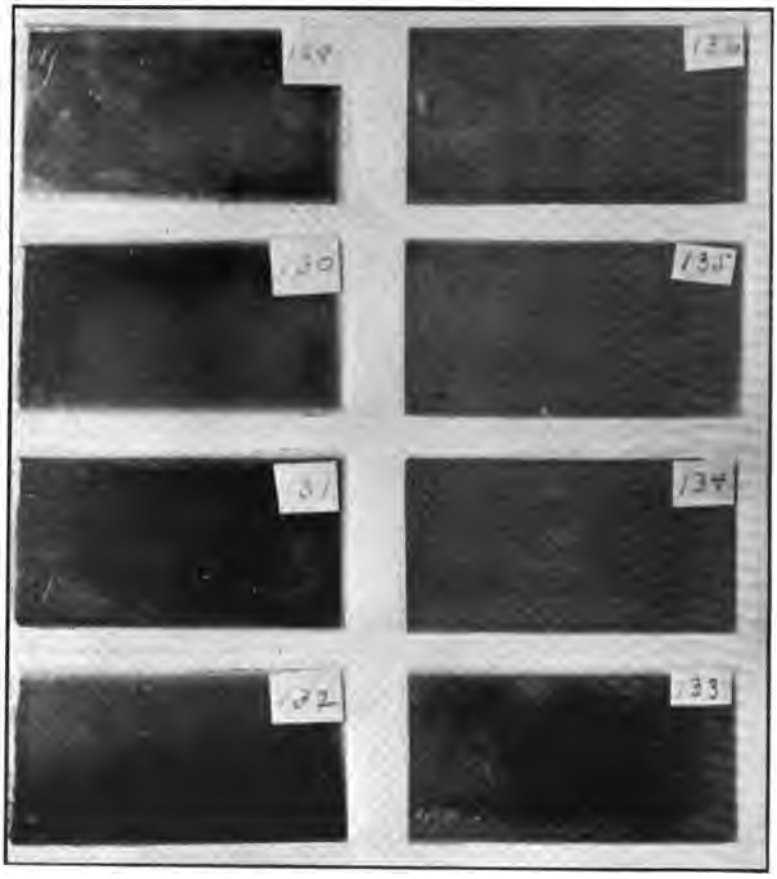
40. *Composition.* 0.10 CuO 0.30 CaO 0.30 K_2O 0.30 ZnO.

A smooth, briliant, glossy glaze, whose surface shows only slight defects in reflected light. Color is very remarkable. Around edges, pale transparent apple-green, showing dark in cut. Then comes a border, one-quarter inch wide, of bright azure blue, semi-transparent and contain fibrous suspended matter, which can be seen in cut. The center is a uniform opaque blue-green or "robin's egg blue." The color seems accidental, and if it could be duplicated might be useful.

---

41. *Composition.* 0.10 CuO 0.30 CaO 0.30 K_2O 0.30 Mgo 0.20.

A gray slightly greenish unfused and rough surface with wide shrinkage cracks. No utility.


129
130
131
132
133
134

Series Fired at Cone 2.

Formula $\left.\begin{array}{l}0.10\ CuO\\0.80\ CaO\\0.15\ K_2O\\0.30\ CaO\\0.15\ ZnO\end{array}\right\} x\ Al_2O_3.\ \ 1.60\ SiO_2$

129. $Al_2O_3 = 0.10$ A fine brilliant copper-green glaze, perfectly transparent and free from blemishes, except slight undissolved matter along lower edge.	136. $Al_2O_3 = 0.45$ A paler green than any. Dapple marks very fine indeed. Glaze is still smooth and perfect for a matt, but the marks of recently healed shrinkage cracks show that the glaze is near its workable limit.
130. $Al_2O_3 = 0.15$ Same as above, without the undissolved matter.	135. $Al_2O_3 = 0.40$ A somewhat lighter colored but equally beautiful matt glaze, very soft and silky in texture. Dapple marks are much finer, but still distinguished in cut. As good as 134 for use.
131. $Al_2O_3 = 0.20$ Darker, yellower green. Glaze was probably dipped heavier than preceding, but color is also different.	134. $Al_2O_3 = 0.35$ A beautiful matt glaze, smooth as silk, with a delicate whitish dapple mark. This glaze is very promising for general matt purposes.
132. $Al_2O_3 = 0.25$ About like 131, only the glaze has drained to lower edge, perfectly transparent.	133. $Al_2O_3 = 0.30$ A yellower green, semi-opaque and with a good deal of a whitish segregation around edges and left hand end. The surface still bright shows dark in the cut.

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RIES, HEINRICH, PH. D., Ithaca, N. Y.	Associate Professor of Economic Geology, Cornell University.
STOVER, EDWARD C., Trenton, N. J.	Trenton Potteries Company.
WALKER, FRANCIS W., Beaver Falls, Pa.	Beaver Falls Art Tile Company.
WATTS, ARTHUR S., New Lexington, Ohio.	Supt., New Lexington High Voltage Porcelain Company.
WHEELER, HERBERT, A., E. M., St. Louis, Mo.	Consulting Engineer, Mining Metallurgical and Ceramic Work.
ZIMMER, WILLIAM H., PH. D., Coburg, Germany.	Redaction Sprechsaal.

ASSOCIATE MEMBERS.

ALRICH, JOHN C., Pittsburg, Pa.	Star Encaustic Tile Company.
ASHLEY, HARRISON EVERETT, Box 291, Leetonia, O.	Chemist of the Salem Iron Co.
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BLOOMFIELD, CHARLES, A., 58 Quincy St., Brooklyn, N. Y.	Dealer in Clays.
BOWMAN, ROBERT K., Trenton, N. J.	Treas., Trenton Fire Clay & Porcelain Company.
BOWMAN, WILLIAM, J. J., C. E., Trenton, N. J.	Trenton Fire Clay and Porcelain Company.
BRIEGLEB, OTTO WILH. GUSTAV, Amsterdam, Holland.	Manufacture of Earthenware.
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BURR, FRANCIS T., 150 Nassau St., New York, N. Y.	Mgr., Richmond Kaolin Company.
CAMPBELL, AUGUSTINE R., Tottenville, Staten Island, N. Y.	Atlantic Terra Cotta Company.
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DEFEBAUGH, EDGAR H., Louisville, Ky.	Editor and Publisher, "Rock Products."
DIEDRICK, WILLIAM H., Salineville, Ohio.	Pres. and Gen. Mgr., The Dresden China Company.
EDWARDS, CLARENCE J. B., SC., Newberg, Oregon.	Newberg Press Brick Company.
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ESKESEN, THROGE R., Matawan, N. J.	The New Jersey Architectural Terra Cotta Company.

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FORST, ARTHUR D., B. S., Morrisville, Pa	National Tile Company.
FRANK, S. ETTINGER, 6-8-10 Bridge St., New York, N. Y.	President, The Schwarz System Brick Company.
FRANZHEIM, CHARLES, Wheeling, W. Va.	Wheeling Potteries Company.
FRERICHS, WILLIAM D., Tottenville, Staten Island, N. Y.	Atlantic Terra Cotta Company.
GATES, ELLIS DAY, Terra Cotta, Ill.	American Terra Cotta and Ceramic Company.
GATES, WILLIAM P., Terra Cotta, Ill.	American Terra Cotta and Ceramic Company.
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GORSLINE, WILLIAM HENRY, 334 East Ave., Rochester, N. Y.	Treas. and Ass't Supt., Rochester Sewer Pipe Company.
GRAY, THOMAS, East Boston, Mass.	New England Pottery Company.
GRUEBY, WILLIAM H., Boston, Mass.	Pres. and Gen'l Mgr., Grueby Faience Company.
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HARDY, ISAAC ERNEST, Momence, Ill.	Supt. Tiffany Enameled Brick Company.
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HASBURG, JOHN W., 588 Fullerton Ave., Chicago, Ill.	Dealer in Ceramic Materials.
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HAUGH, HARRY, Anderson, Ind.	Supt., Columbia Encaustic Tile Company.

HENSEL OTTO, Chicago Heights, Ill.	Ludowici Roofing Tile Company.
HICE, RICHARD, R., Beaver, Pa.	Fallston Fire Clay Company.
HULL, WALTER A., Bolivar, Pa.	Reese-Hammond Fire Brick Company.
HUNT, WILLIAM H., Cleveland, O.	Cleveland Hydraulic Pressed Brick Company.
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JUNGE, WILLIAM H., Chicago, Ill.	Northwestern Terra Cotta Co.
KOCH, CHARLES FREDERICH, Covington, Ky.	Cambridge Tile Manufacturing Co.
KREHBIEL, JUNIUS, F., Delevan, N. Y.	Student, New York School Clay Working and Ceramics.
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LANDERS, WILLIAM FRANKLIN, Indianapolis, Ind.	Supt., U. S. Encaustic Tile Works.
LINDEROTH, SVEN, Chicago, Ill.	Alhambra Ceramic Works.
LOCKE, FREDERICK, M., Victor, N. Y.	Manufacturer Porcelain Insulators.
LUDLOW, STEPHEN H., Parma, Mich.	Chemist, Peninsular Portland Cement Company.
MADDOCK, ARCHIBALD M., Trenton, N. J.	Thomas Maddock & Sons.
MADDOCK, THOMAS, Trenton, N. J.	John Maddock & Sons.
MADDOCK, JOHN, Trenton, N. J.	John Maddock & Sons.
MANDLE, I., 3943 Pine St., St. Louis, Mo.	Miner and Shipper of Pottery Clay.
MCBEAN, ATHOLE, 1358 Market St., San Francisco, Cal.	Sec'y and Treas., Gladding, McBean & Company.
MIDDLETON, JEFFERSON, Washington, D. C.	Statistician, United States Geological Survey.
MORRIS, W. C., St. Louis, Mo.	Vice Pres. and Gen. Mgr., Christy Fire Clay Company.
MOSES, JAMES, Trenton, N. J.	Mercer Pottery Company.

OLSEN, NORMAN, Palermo, Sicily.	Ass't Mgr., The Manifattura Ceramica Florio.
PARKER, LEMON, St. Louis, Mo.	Parker-Russell Mining and Manufacturing Company.
PARMELEE, CULLEN W., New Brunswick, N. J.	Director Dept. Ceramics, Rutger's College.
PLANT, ALFRED, Keyport, N. J	Eagle Tile Company.
POPE, CHARLES A., Trenton N. J.	Trenton Fire Brick Company.
POST, MALCOLM P., St. Louis, Mo.	Foreman, Blackmer & Post, Sewer Pipe Company.
POWELL, WILLIAM H., New York, N. Y.	Excelsior Terra Cotta Company.
PURINGTON, DILWIN V., Chicago, Ill.	Pres. Purington Paving Brick Co.
RAMSEY, ANDREW, Mt. Savage, Maryland.	Mt. Savage Enameled Brick Co.
RANDALL, THEODORE A., Indianapolis, Ind.	Editor "Clay-worker."
RAUTER, DR. GUSTAV, Charlottenberg 4, Berlin.	Engineering Chemist.
REA, WILLIAM J., Buffalo, N. Y.	Supt. Buffalo Pottery Company.
RIX, WILTON PARKER, 84 Liverpool Road, Newcastle, Staffordshire, England.	Consulting Ceramist.
ROBINEAU, SAMUEL E., Rochester, N. Y.	Editor, "Keramic Studio."
ROESSLER, FRANZ, 100 William St., New York, N. Y.	Vice Pres. and Supt., Roessler & Hasslacher.
RUSSELL, JOSEPH, 1802 Queen St., East, Toronto, Canada.	Brick Manufacturer.
SANT, JOHN, East Liverpool, Ohio.	Clay Salesman, Miner & Broker.
SIMPKINS, RALPH, St. Louis, Mo.	Mgr. Union Pressed Brick Works.
SKEELE, H. B., Chicago, Ill.	Ludowici Roofing Tile Company.
SMITH, AMEDEE M., Portland, Oregon.	Western Clay Manufacturing Co.
SPEIR, OSWALD, Perth Amboy, N. J.	Ass't Gen'l Mgr., Perth Amboy Terra Cotta Company.

STAMM, JOHN, East Liverpool, Ohio.	General Manager National China Company.
STANBERY, GEORGE A., Zanesville, Ohio.	American Encaustic Tile Company, Ltd.
SWEET, GEORGE A., F. G. S., Brunswick, Victoria, Australia.	"The Close," Wilson St.
TAYLOR, WILLIAM WATTS, Cincinnati, Ohio.	President Rookwood Pottery Co.
THOMAS, DAVID C., Wellsville, Ohio.	United States Pottery Company.
TOWNSEND, EVERETT, Morrisville, Pa.	National Tile Company.
TROMMER, CARL E., New York, N. Y.	Roessler and Hasslacher Chemical Company.
VAN BRIGGLE, ARTUS, Colorado Springs, Colo.	Pres. Van Briggle Art Pottery Co.
VOELCKER, GUSTAV, Lancaster N. Y.	Lancaster Face Brick Company.
WAGNER, FRITZ, Chicago, Ill.	Northwestern Terra Cotta Co.
WALKER, JOHN A., Jersey City, N. J.	Joseph Dixon Crucible Company.
WEELANS. CHARLES, Trenton, N. J.	Supt. Monument Pottery Co.
WELCH, JOHN H., 3285 Morganford Road, St. Louis, Mo.	The Mound City Roofing Tile Co.
WHITEHEAD, C. LOUIS, Trenton, N. J.	Providential Tile Works.
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WRIGHT, HORACE BERTRAM, Box 11, Bolivar, Pa.	Reese-Hammond Fire Brick Co.
YATES, ALFRED, Johnsonburg, Pa.	Johnsonburg Vitrified Brick Co.
YOUNGREN, PETER LEWIS, 261 15th St, Milwaukee, Wis.	Kiln Builder and Burner.

RULES.

OBJECTS.

The objects of the American Ceramic Society are to promote the arts and sciences connected with Ceramics by means of meetings for social intercourse, for the reading and discussion of professional papers, and for the publication of professional literature.

MEMBERSHIP.

The Society shall consist of Honorary Members, Members and Associates.

Honorary Members must be persons of acknowledged professional eminence, and shall not exceed five in number.

Members shall be persons competent to fill responsible positions in Ceramics and have suitable qualifications.

Associates shall include persons interested in Ceramics and the allied arts.

Honorary Members shall be proposed by at least five Members, approved by the Council, and receive at least 90 per cent. of the votes cast by letter ballot at the annual meeting.

Members and Associates shall be proposed by at least three Members or Associates, approved by the Council, and receive at least 75 per cent. of the votes cast by letter ballot. A candidate for admission must make application on a form prepared by the Council which shall contain a written statement of his age, professional experience, and that he will, if elected, conform to the laws, rules, and requirements of the Society.

All Honorary Members, Members and Associates shall be equally entitled to the privileges of membership, except that only Members shall be entitled to hold office and to vote. Applicants for a change in grade of membership shall conform to the requirements of a new applicant.

Any person can be stricken from the membership of the Society on the request of five or more Members, on the recommendation of a majority of the Council, if he fails to resign on the advice of the Council. Such person, however, shall first be notified of the charges against him, and be given a reasonable time to appear before the Council, or present written defense, before final action is taken by the Council.

DUES.

Honorary Members shall be exempt from all dues.

The initiation fee of Members shall be ten dollars, and of Associates five dollars, which if not paid within six months after election, will render the election void.

The annual dues for Members will be fixed by the Council, but shall not exceed five dollars per year.

The annual dues for Associates will be fixed by the Council, but shall not exceed four dollars per year.

Any Member or Associate in arrears for over one year may be suspended from membership by the Council until such arrears are paid.

OFFICERS.

The affairs of the Society shall be managed by a Council, consisting of a President, Vice President, Secretary, Treasurer, and three Managers, who shall be elected from the members at the annual meeting, and hold office until the adjournment of the meeting at which their successors are elected.

The President, Vice President, Secretary and Treasurer shall be elected for one year, and the Managers for three years; and no President, Vice President, or Manager shall be eligible for immediate re-election to the same office.

The duties of all officers shall be such as usually appertain to their offices, or may be delegated to them by the Council or the Society; and the Council may at its discretion require bonds to be furnished by the Treasurer.

Vacancies in any office shall be filled by appointment by the Council, but the new incumbent shall not thereby be rendered ineligible to re-election at the next annual meeting to the same office. On the failure of any officers to execute his duties within a reasonable time, the Council, after duly warning such officer, may declare the office vacant, and appoint a new incumbent.

A majority of the Council shall constitute a quorum; but the Council shall be permitted to carry on such business as it may desire by letter.

ELECTIONS.

At the annual meeting, a nominating committee of five Members, not officers of the Society, shall be appointed, and this committee shall send the names of nominees to the Secretary at least 60 days before the annual meeting, who shall immediately forward the same to the Members. Any other five members may also present the names of any candidates to the Secretary, provided it is done at least 30 days before the annual meeting. The names of all candidates, provided their assent has been obtained, shall be placed on the ballot without distinction as to nomination by the regular or an independent nominating committee, which shall be mailed to every member, not in arrears, at least 20 days before the annual meeting.

The ballot shall be inclosed in an inner blank envelope, and the outer envelope shall be endorsed by the voter, and mailed to the Secretary for collection. The blank envelopes shall be opened by three Scrutineers appointed by the Chair at the annual meeting, who will report the result of the election. A plurality of the votes cast shall elect.

MEETINGS.

The annual meetings shall take place at such time and place as the Council may decide, at which time reports shall be made by the Council, Treasurer, and Scrutineers of election, and the accounts of the Treasurer, audited by a committee of three appointed by the Chair.

Other meetings may be held at such times and places during the year as the Council may decide, but at least 20 days' notice shall be given of such meetings.

Seven Members shall constitute a quorum at any regular meeting, and a majority shall rule, except where otherwise specified.

The order of business at the annual meeting shall be:—

1. Reading of Minutes of last meeting.
2. Reports of the Council and Treasurer of the Society.
3. Announcement of Election of Members.
4. Announcement of the Election of Officers.
5. Appointment of Nominating Committee.
6. Old Business.
7. New Business.
8. Reading of Papers.

PUBLICATIONS.

The Council shall act as a Publication Committee, and decide as to what to publish. The publications of the Society shall be sent to all Members and Associates not in arrears The Secretary shall furnish each author with reprints of his papers at cost price, provided due notice is given that reprints are desired.

The Society is not, as a body, responsible for the statements or opinions expressed in its publications.

PARLIAMENTARY STANDARD.

Roberts' "Rules of Order" shall be the parliamentary standard on all points not covered by these rules.

AMENDMENTS.

These rules may be amended at any regular meeting by a two-thirds vote of a letter ballot at the subsequent annual meeting provided a written notice of such proposed change is sent to each Member at least 30 days before said annual meeting. Said proposed amendments shall be printed on the ballot for officers and counted by the same Scrutineers.

PUBLICATIONS.

The publications of the Society are as follows:

DESCRIPTION OF VOLUME.		Price to Members.	Price to Others.
Vol. I.	Transactions for 1899, 110 pages, bound in paper,	$0.50	$4.00
Vol. II.	Transactions for 1900, 278 pages, bound in paper,	2.00	4.00
Vol. III.	Transactions for 1901, 230 pages, bound in paper,	1.00	4.00
Vol. IV.	Transactions for 1902, 300 pages, bound in paper,	1.50	4.00
Vol. V.	Transactions for 1903, 420 pages, bound in paper,	2.00	4.00
Manual of Ceramic Calculation (contained in Vol. II as a part thereof,) 86 pages, bound in paper,		1.00	1.00
The Collected Writings of Dr. Hermann August Seger, Volume I. Contains (A) Treatises of a general scientific nature, (B) Essays relating to Brick and Terra Cotta, Earthenware and Stoneware, and Refractory Wares. Pages, 552. Bound cloth,		7.50	7.50
The Collected Writings of Dr. Hermann August Seger, Volume II. Contains (B) Essays on Whitewares and Porcelain. (C) Travels, Letters and Polemics. (D) Uncompleted works, and extracts from the archives of the Royal Porcelain Factory. Pages, 610 (est). Bound in cloth,		7.50	7.50

Every member or associate receives one copy of the Transactions free for each year for which he pays dues. He is entitled to purchase copies for the years antedating his connection with the Society at the costs indicated in the first column. All others can obtain copies at the prices listed in the second column by sending order, accompanied with check, to

STANLEY G. BURT,

Care of Rookwood Pottery Co.,

CINCINNATI, OHIO.

PREFACE.

The following pages constitute the record of the Society's fifth year of work. The meeting at Boston was the most largely attended of any yet held, in spite of the location being so far from the center of our geographical distribution. Also, the programme was longer, and more fully occupied than ever before. The trips and visits made by the Society added greatly to the value and enjoyment of the meeting.

The policy of publishing papers not yet read, or read by title only, has been continued and extended. Half of the present volume is of this sort, and some of the ablest papers are in this list. Also, many of those read were not discussed at the meeting for lack of time and lack of preparation of the members, and have been discussed in writing since.

The problem immediately before the Society is still much the same as one year ago. We need to diminish the time spent in listening to the reading of papers, and to increase the time spent in discussing those which we have had opportunity to read and study in advance. The first step in this direction was taken in the case of Mr. Fiske's paper this year. The results have encouraged the Council to make the following offer, viz.: Those papers sent in to the Secretary thirty days before a meeting will be printed and distributed in advance, to be used as a basis of prepared discussions. These discussions will be given the preference on the programme, other things being equal, over papers not printed in advance.

This step, it is confidently believed, will do much to strengthen the work of the society. The discussions of Mr. Fiske's paper, and also Mr. Hull's paper on Chrome Tin Pink may be taken as indications of what may be expected.

The work of the Society, as evidenced by the volume, shows a progressively broadening scope. Not only is the quantity greatly in excess of previous years, but also the variety of topics treated and the amount of new and independent research is equally increased. The field of the hydrous silicates especially has been greatly developed by the two important researches now published, and the fields of the slags and glasses have received attention in our literature for the first time, thus leaving no large field of silicate technology wholly untouched.

It is a pleasure to state that the general condition of the Society, judged by the cruder tests of increasing membership, broadening geographical range, and financial solvency, is most excellent.

EDWARD ORTON, JR.,
Secretary.

Columbus, O., July 20, 1903.

PLATE 1.—EXTERIOR VIEW OF FACTORY.

THE ELIMINATION OF HAND LABOR IN BRICK MAKING.

BY

J. PARKER B. FISKE, B. Sc., BOSTON, MASS.

All observing persons will agree that the manufacturing world has practically been revolutionized within the last quarter of a century by the use of labor-saving machinery. Hardly a class of manufacture exists that has not felt its magic touch, and with its aid the United States has become the foremost manufacturing country in the world. A manufacturer in nearly every industry who should return to the practice of even ten years ago in preference to that of today would invite failure and bankruptcy.

This is the age of machinery — automatic machinery, often with the most intricate mechanism, which works with marvelous speed and accuracy, and which accomplishes tasks far beyond the capability of the human hand, both in quality and quantity of product manufactured. As a result of such machinery, the cost of nearly all manufactured articles has been enormously reduced and the consumption by the people correspondingly increased.

It is the purpose of this paper to discuss the application of machinery to the art of brick making, with a view to the practical elimination of hand labor.

Much ingenious and useful brick machinery has already been invented and is in daily use in hundreds of plants; this is, however, largely confined to the preparation and handling of the clay and the forming of the soft bricks. This machinery has been perfected to such a point that little improvement is needed or can be expected in future, and the same remarks are doubtless true of the most modern types of drying and burning devices.

No successful attempt has been made, however, so far as I am aware, to eliminate the manual labor involved in handling the bricks from the brick machine through the

various processes of drying, burning, storing and delivery to the user, and inasmuch as this labor of handling constitutes a large part of the entire labor involved in the manufacture, comparatively little saving has been effected in the total labor cost.

Certain improvements, such as the various car and truck systems, have come into use to a large extent, but these have been adopted mainly because of their convenience and adaptability to the artificial drier rather than as labor-saving devices pure and simple. These devices must still be propelled over long distances by man power and the bricks, even under the most favorable conditions, must undergo repeated handlings in setting and emptying the kilns and in loading the delivery cars or wagons.

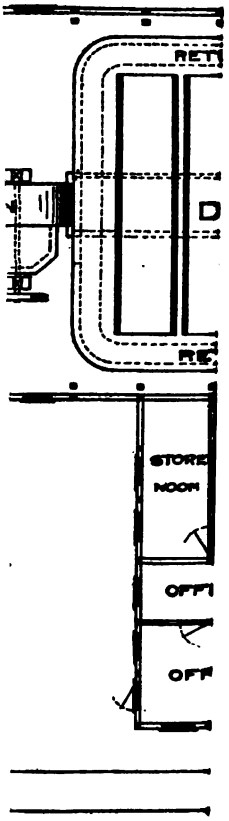
The number of hand operations in modern brick making varies greatly, depending upon the process, whether molded by the "soft mud" or the "stiff mud" machines, whether dried on the open yard in racks or in artificial driers, and whether burned in up draft, down draft or continuous kilns. It is fair to say, however, that an average of all plants would show from 12 to 15 hand or man power operations, from the time the bricks are molded until they are loaded on the delivery wagons.

When the value per ton of the finished product is taken into account, this amount of hand labor appears excessive; thus a ton of common bricks is worth on the average say two and a half dollars on the wagon or cars at the plant, while a ton of steel rails at twenty dollars has excited the wonder of the whole world. Such a low value should involve a minimum of hand labor per unit of weight, yet it is doubtless true that no finished product in the world involves so much.

In short, an excessive proportion of the entire cost of brick making today is in the labor item, and our only hope of substantial reduction lies in the minimizing of this hand labor.

The prime necessity for the successful use of automatic machinery in any art is that every unit of manufacture shall be subjected to precisely the same treatment, and this

[redacted] brick



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condition obtains to an ideal extent in the art of brick making. Thus, particularly in the case of common bricks, every piece is molded or pressed exactly like its mates, and is subjected to the same drying and burning heats and, theoretically, when finished should be of exactly the same size, shape and texture.

However, a careful study of this problem will, I think, convince anyone that no great improvement can be made in the "summer brickyard;" that is, one in which the drying is done by sun and wind, as these elements are intermittent in their action and are so uncertain at best that manufacturing can be conducted not over one-third of the total number of working days in the year. It is also important for the profitable use of labor-saving machinery that it shall be worked systematically and continuously in order that it may have an opportunity to earn a fair interest on its first cost, which is necessarily high. To make any radical improvements, therefore, the "open yard" idea must be abandoned and a permanent, substantial construction must be adopted, with the best possible form of brick molding machinery, artificial driers and fuel saving kilns, specially constructed to suit the new conditions involved in the use of automatic handling machinery, the whole plant representing a comprehensive scheme complete in all its arrangements, so that the raw material may enter at one end, proceed systematically through the various processes and emerge a finished product at the other, practically without hand labor.

Having perfected such a system and put it into successful operation at the works of The Fiske Brick Co., at Dover Point, N. H., I take pleasure in presenting a description of it to this society.

Plate 1 shows a photograph of the exterior of the main building, with monitor roof, 367 feet long and 60 feet wide, and the auxiliary building being 115 feet long and 50 feet wide.

Plate 2 is a plan view of the plant thus far erected, and shows:

Power Plant, consisting of two tubular boilers, pumps and feed water heater, a 200 H. P. Corliss engine, 50 kilowatt

belted dynamo, 50 kilowatt direct connected engine and dynamo as a spare unit, switchboard, shafting, etc.

Brick Machinery, consisting of a Chambers disintegrator and a Freese combined pugmill and auger machine for making stiff mud bricks, with an automatic cutter and an off-bearing belt extending nearly across the main building.

Setting-Up Stands for the reception of the green bricks as they are taken from the belt.

Drying Arrangements, consisting of a Crown Heat Generator, a twelve-foot Sturtevant fan and four drying chambers of special construction, as described later.

Kiln, consisting of thirteen chambers arranged in one battery for burning by the continuous or regenerative principle.

Plate 3 is a bird's-eye interior view looking north, and shows the storage rack and drier in the foreground.

The clay is drawn up an inclined trestle in side dumping cars, each of five yards capacity, and is dumped into a hopper at the disintegrator. The material proceeds through the disintegrator, brick machine and cutter, and the green bricks pass out into the main building on the off-bearing belt, all in the usual manner.

At this point in the manufacture the special handling machinery begins its work, the process thus far being an entirely standard one.

Careful consideration of the conditions necessary for the drying and burning of bricks has convinced me that the successful handling machinery must handle the bricks in large masses rather than one by one, as is done by labor saving machinery in other arts, and I consequently introduce at this point in the manufacture what I term the "unit stack" idea, which I believe to be entirely novel in brick making.

The unit stack consists of a pile of bricks built up, as they issue from the molding or pressing machine, in such relative position, brick to brick, that they can be dried, burned and loaded on the delivery car or wagon without rearrangement, the entire stack being lifted as a unit, transported and deposited by suitable machinery, first into

PLATE 3.—BIRDS EYE VIEW OF INTERIOR OF MAIN BUILDING, SHOWING STORAGE RACK FOR DRIED BRICK IN THE FOREGROUND.

the drier, then into the kiln, and finally into the stockyard or delivery wagon. The original stacking can be done automatically, layer upon layer, by machinery, or can be done by hand as in the plant under discussion.

The unit stack system creates a distinct advance in the art, as it eliminates all manual handling after the bricks are once stacked, banishes from the brick factory all tip carts, trucks, wheel-barrows, drier cars, tracks, transfer cars and turn-tables, dispenses with kiln "setters" and "emptiers," and reduces the entire force of employes, beyond the brick machinery gang proper, to a mere handful of men.

In order to place the bricks in unit stack formation, a rack called a "setting-up stand" is permanently located on each side of the off-bearing belt, and at a suitable distance therefrom to accommodate the stackers.

Plate 4 shows the sitting-up stands, constructed with 105 heavy horizontal bars of hard pine, mounted at a convenient height for the stacking of the bricks, these bars being spaced apart so as to support each lower brick at its two ends and in the middle, leaving two clear spaces about 1½ inches wide under each bottom row. The bricks may be set five over two, six over two, or in any manner required for their proper drying and burning, exactly as is done in the usual plant, and no more care is required than in setting bricks in an ordinary kiln. For these reasons a very wide variety of clays can be handled by this system, as any style of setting may be adopted which is found necessary to suit the peculiarities of the clay in question.

The stand is about 39 feet long by 20 inches wide, and holds about 1,500 bricks when stacked five over two (that is, five headers over two stretchers) and six courses high. At the back of the stand are vertical guide bars, also of hard pine, to insure a perfectly vertical stack and to guide the stackers in setting the top row of bricks, which must be spaced exactly to correspond with the horizontal bars, as will be seen later.

As soon as the first stack is completed, the stackers commence placing the bricks on the opposite stand, the first stack being soon removed by the handling machinery

thus leaving the first stand empty and ready for another stack. Thus all waiting for empty cars or trucks, as is common in many brick plants, is eliminated, and the brick machine is free to run without interruption. Plate 4 shows one stand filled and the other partially filled.

The handling machinery consists of an overhead electric traveling crane and a special carrier called a "brick lift."

By reference to Plate 5 it will be seen that the crane travels on tracks supported on an elevated runway near the eaves of the main building, and that it spans this building completely and travels its entire length. The crane is driven along the overhead tracks by an electric motor called the "travel motor," located midway on the crane girder and shown in the cut, and the hoisting and lowering of the load is effected by the "hoist-motor," located on top; each motor is controlled independently by a lever in the operator's cage. The hoisting is done by four vertical chains and four winding drums driven in unison by the main shaft from the hoisting motor.

The brick lift consists of two main lifting beams properly braced together to form a girder; to this girder are attached 104 lifting fingers accurately spaced apart to correspond to the spaces between the bars of the setting-up stand, so that the whole forms a rigid structure. The four vertical chains already mentioned are attached to four lifting eyes on the lifting beams, thereby securing the utmost stability of the loaded lift, tipping in any direction being impossible and swaying and twisting being reduced to a minimum.

To engage and transport a load of bricks, the lift is brought by the crane in front of the setting-up stand, lowered until the fingers are at the proper level, the crane moved forward until the horizontal members of the fingers are run into the spaces between the bars of the stand and underneath the lower layer of bricks. Plate 6 shows the fingers in the act of entering these spaces. The lift with the entire load of bricks is then raised clear of the stand by the hoisting motor, and is carried by the crane to the drier as a unit.

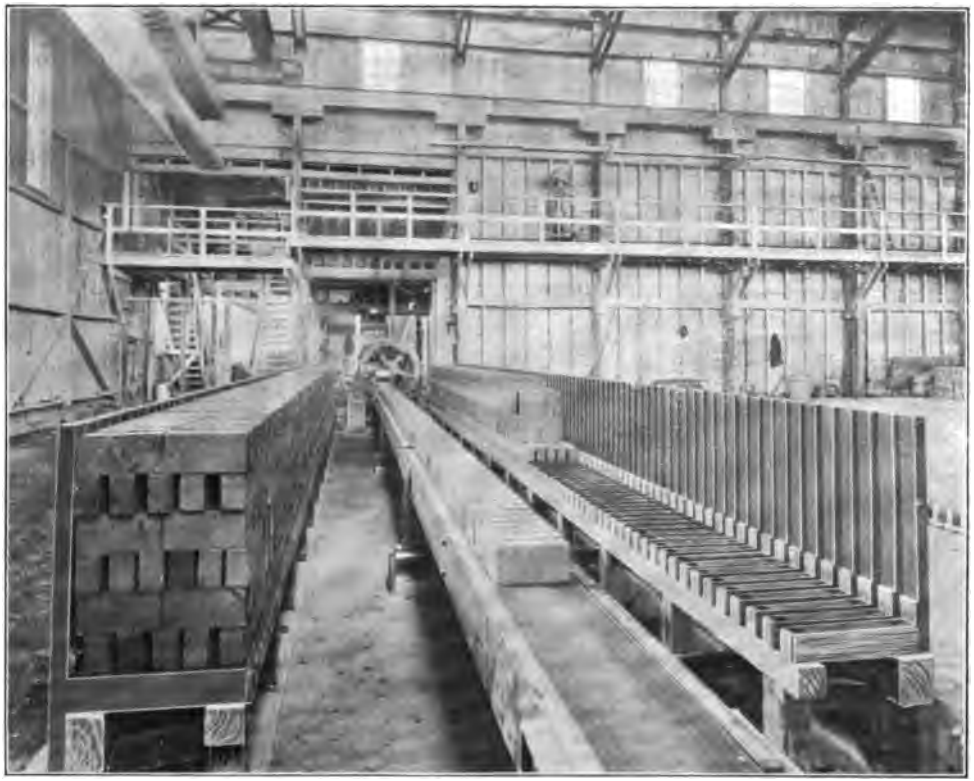


PLATE 4.—SETTING-UP-STANDS, ONE FILLED WITH SOFT BRICKS AND ONE PARTLY FILLED; THE OFFBEARING BELT BEING BETWEEN THEM

By reference to Plate 7, it will be seen that each brick of the bottom layer is supported on two fingers of the lift, hence there is perfect stability of the load and it is impossible for it to twist or topple over. Repeated trials with the most violent movements of the crane that were possible have failed to dislodge a single brick from its load.

The crane operator is located in a cage at one end of the crane, as shown in Plate 5, and at all times has an uninterrupted view of the runway and of his work below.

It will be noted that this operator, with one helper on the brick lift, can handle as many bricks at a time as 15 to 20 men with wheelbarrows, and that the speed of travel along the tracks is faster than a man can comfortably walk.

Handling the unit stack during the remainder of the process is accomplished by the use of racks in the drier, kiln and store room, each constructed with horizontal members accurately spaced for the reception of the bricks as in the case of the setting-up stand and provided with spaces between its members to correspond to the fingers of the lift. Thus in the drier these racks are of steel I beams, in the kiln they are of bricks to withstand the heat necessary for burning the ware, and in the store room they are of either steel or wood.

The drier shown in section in Plates 8 and 9 is of brick and steel construction, is absolutely fireproof and is novel throughout. It is divided into four chambers by permanent partition walls, each chamber being provided with a cover removable in one piece; these covers are constructed of I beams with an upper and lower covering of sheet iron, thereby providing a dead air space of about 4 inches. Plate 10 shows one of these chambers with the cover removed, two stacks of soft bricks having been placed in the chamber.

In the bottom of each chamber is a fixed rack of I beams constructed as already described; the stacks of bricks are brought from the setting-up table and deposited successively upon this rack, the brick lift being lowered through the rack to deposit the bricks, and being backed out and raised from the rack each time after so depositing them.

As the soft bricks will not bear the weight of superimposed loads, each chamber is provided also with two removable racks, which are supported by horizontal channel beams built into partition walls separating the chamber. Each removable rack is built of I beams like the fixed rack below it, and each holds four stacks of bricks, thus making the capacity of each chamber 12 stacks, or about 18,000 bricks.

The arrangement of the covers and the racks with their loads of bricks is clearly shown in Plates 8 and 9.

The method of handling the removable racks is shown in Plates 11 and 12. Each rack is provided on its under side with two flat iron bars running lengthwise and equidistant from the center line of the rack. The fingers of the brick lift are dropped below the level of these bars and are then run under them so that the rack can then be lifted and transported, the bearing on the two bars preventing the rack from tipping or twisting.

The covers are provided with latticed lifting beams attached to the tops so that the fingers of the lift can be run under them for the purpose of handling the covers from one chamber to another. A cover is generally removed from a chamber of dried bricks and placed upon a chamber just filled with bricks to be dried. Each cover, when in position, is supported by channel irons on the top of the partition walls, as shown in Plates 8 and 10, these channels being filled with sand; a downwardly projecting lip on the cover imbeds itself in this sand when the cover is placed in position, thereby automatically sealing the chamber.

Hot air is forced through the bricks by a powerful fan driven by an electric motor, the air passing through three chambers in succession, as shown in Plate 9. Inasmuch as the bricks can not be progressed through successively hotter and hotter zones, as in the case of the tunnel drier, the same results are obtained by progressing the zone of maximum heat through the bricks. Thus the air being heated by a suitable device, in this case by a Crown Heat Generator, it is forced into a circular tunnel running lengthwise of the building and passing under the center of each cham-

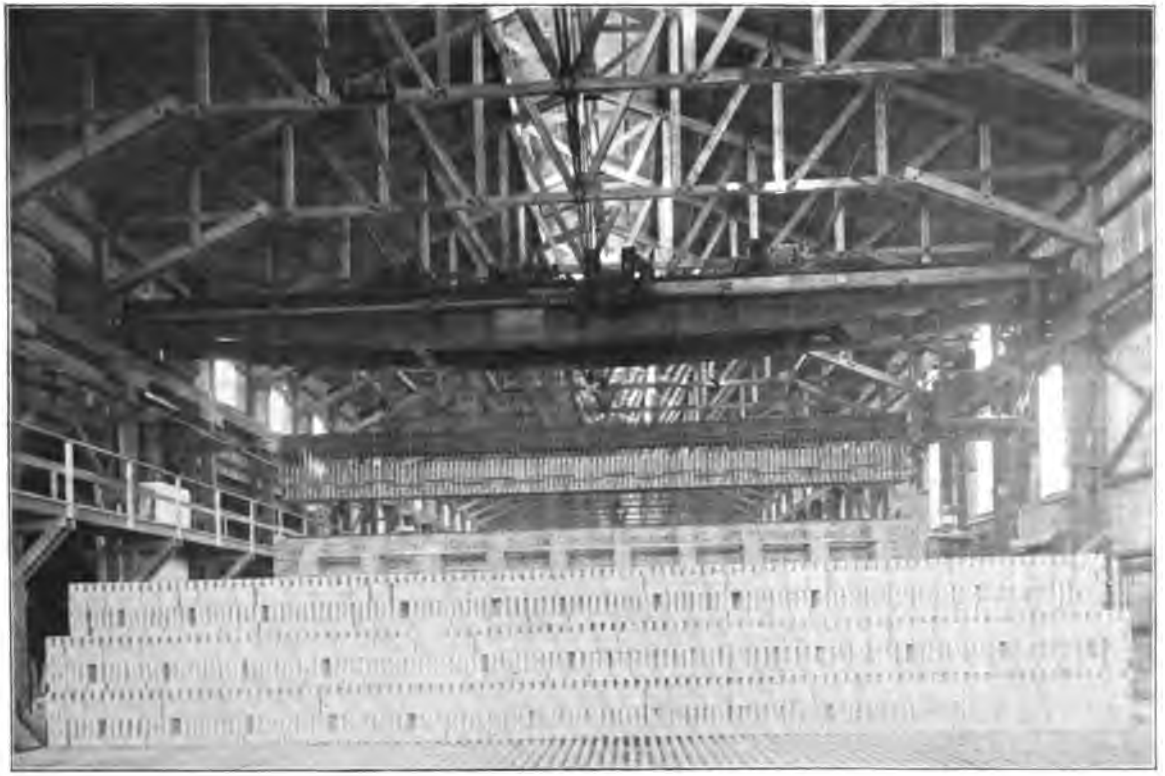


PLATE 5.—SHOWING CRANE TRAVELLING ON ELEVATED RUNWAY AND CARRYING A STACK OF SOFT BRICK TO THE DRYER. DRIED BRICKS IN STORAGE IN THE FOREGROUND.

ber, as shown in Plates 8 and 9. On each side of this tunnel and under each chamber is a large opening, closed by a curved hanging damper controlled by a chain from the outside of the drier. This opening admits just the proper amount of air to a distributing compartment located beneath the lower brick rack and provided with a sloping bottom to insure a proper flow of air into all parts of the drying chamber. The curved dampers being closed under all the chambers except the one of nearly dried bricks, the air passes from the tunnel into the distributing compartment, and thence upward through the bricks.

In order to facilitate the proper circulation, a wide space is left between the bricks at one side of the chamber and the partition wall, this space being insured by the room necessary for backing out the brick lift after depositing its last load on each rack. In the case of the lower rack, a wide sheet iron floor is attached to the lower side of the rack, underneath that portion covered by the bricks, in order to force all the air first into the clearance space and up to the cover, and thence downward and at more or less of an angle through the bricks to openings or wickets in the base of the opposite partition wall, and thence into the next chamber. To prevent the air from passing directly through the lower tier of bricks to the wickets, canvas baffle curtains are hung from the bottom of the two movable racks, thereby forcing the air up, over and then downward through the bricks. The wickets are closed by vertical sliding dampers, controlled by chains passing upward through 2-inch pipes imbedded in the partition walls.

The hot air is first admitted to a chamber of nearly dried bricks and passes thence to a partially dried chamber, thence to a chamber freshly filled and finally into an open chamber just being filled. As soon as the first chamber has been thoroughly dried and the last chamber has been filled, the cover of chamber No. 1 is removed, and is placed in position on No. 4, the hot air is shut off from the No. 1 and admitted directly to No. 2, which by this time has become sufficiently heated and dried to prevent checking of the ware. Chamber No. 1 is now ready to have its brick re-

moved to the kiln for burning. Chamber No. 4 is connected with Chamber No. 1 by return flues, located on either side of the drier in order that the chambers may operate in rotation without any interruption, as in the case of the continuous burning kiln.

Thus the zone of maximum heat is progressed from chamber to chamber, the nearly dried brick receiving the maximum heat and the green bricks being subjected to warm moist air as in the most approved types of progressive artificial driers.

The bricks are carried from the drier to the kiln by the crane and brick lift, and are there deposited, each chamber (holding about 18,000 bricks) having a removable crown, so as to leave the entire top open. The bottom of the kiln, or that portion covered by bricks to be burned, is provided with a special fire brick floor with separated ribs, corresponding in spacing to the fingers of the brick lift, so that the stack of dried bricks is deposited without difficulty and the lift withdrawn, as in the case of the drier, each brick of the lower layer being supported by the ribs of the floor at its two ends and in the middle. Plate 13 shows one kiln dried brick, and Plate 14, taken during the construction of the kiln and before the partition walls were built, shows the brick lift in the act of depositing a load of bricks on the ribs forming the special kiln bottom.

After the four stacks of bricks forming the lower tier have thus been placed in position in the kiln, other successive stacks have been placed upon them, the fingers of the lift passing down into the spaces of the upper row of bricks of the tier below. It will be noted that the upper row of bricks of each stack is spaced especially wide apart to permit this operation, as has already been described.

The setting consists of three tiers of four stacks each. After placing the last stack in position the empty lift is backed out into the "combustion space," and is then withdrawn from the kiln. In reverse manner, the lift enters the kiln when the bricks have been burned and cooled and removes one load after another to the point of shipping or storage.

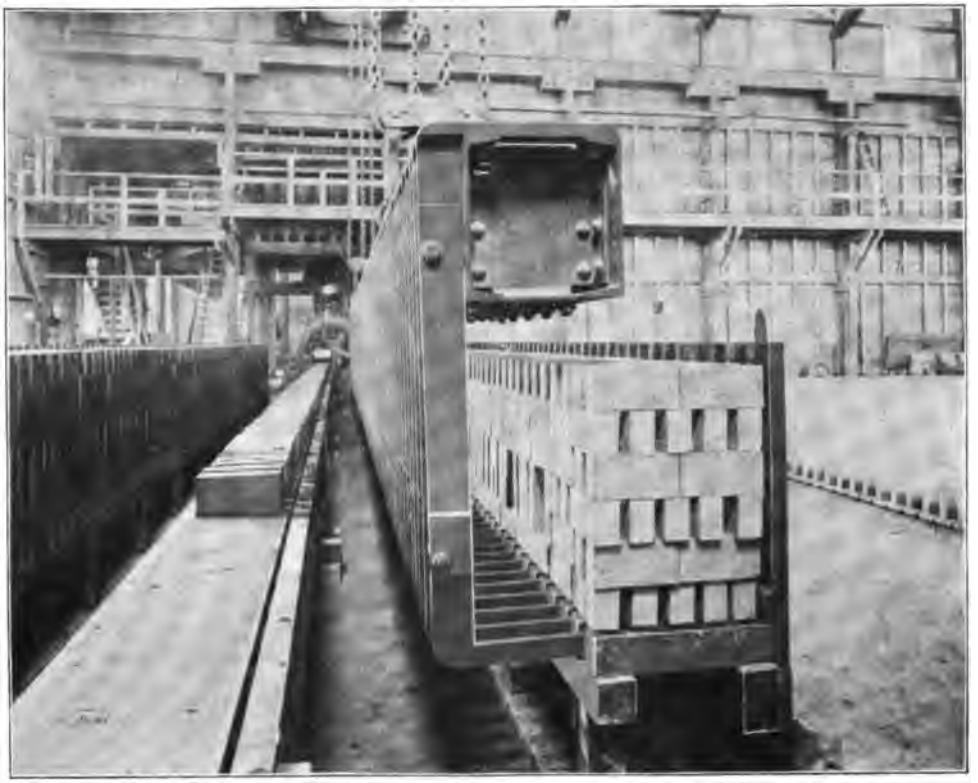


PLATE 6.—SHOWING THE FINGERS OF THE BRICK LEFT IN THE ACT OF ENTERING SPACES OF THE RACK, PREPARATORY TO LIFTING THE BRICK.

Plate 15 shows a kiln chamber filled with bricks ready for the reception of the portable crown.

The kiln operates on the "semi-continuous" principle, and is built with permanent transverse partitions, dividing it into chambers; the fires are fed with coal through holes in the crown and are supplied with hot air from the cooling chambers behind, the air entering the burning chamber through the "wickets" in the base of the partition wall; the waste heat of the burning chamber passes forward in a similar manner to heat up the green bricks in advance of the fires. Thus the utmost economy of fuel is secured.

Many novel features are to be noted in the construction and operation of the kiln under discussion.

The chambers are unusually low and narrow, which enables them to be fired largely from one side and to produce a very uniform burn.

The open space above mentioned acts not only as a combustion chamber, but provides an entrance and exit for the brick lift to the chamber when the latter is filled with bricks.

The removable crowns for the kiln chambers are constructed of fire clay blocks and cast iron hollow beams of special shape and section. These beams are placed about 18 inches apart by separating bolts and span the kiln chamber from one partition wall to the next, the ends resting on special cast iron channel irons anchored along the top of the partitions. The beams support heavy blocks, made of high grade fire clay mixed with sawdust to give porosity, the soffits of the beams being amply protected by fire clay key pieces.

The crown is sealed around the edge with soft bricks and sand and gives a construction of great superiority over the ordinary mason work arch. No lateral thrust of any kind on the side walls or partition walls is produced as the crown expands and contracts, the iron beams simply sliding on the channel iron supports. Moreover, repairs may be made to a crown by removing one or more fire clay blocks without tearing down the whole crown, and without interrupting the operation of the kiln, as is often

the case in ordinary fire brick construction. The crowns are lifted, transported and deposited by the crane and the brick lift, which engages a heavy latticed lifting girder attached to each iron beam of the crown. This construction provides a liberal amount of flexibility and secures a satisfactory seating of each beam on the wall without undue straining of the fireproofing materials. The construction of the crowns and the manner of lifting them are shown in Plate 16.

As all filling and emptying of the chambers are done through the top, the kiln is built with thick, permanent and unbroken side walls, all arched doorways being omitted. Thus the necessity of continually bricking up and tearing down the doors, as in other kilns, and the usual complement of soft burned bricks near the doorways are obviated. Moreover, as the side walls are unbroken from end to end, it is possible to build the kiln with all flues in the walls, thereby eliminating the underground flue absolutely. As is well known, underground flues are often cold and damp and are a source of great irregularity of draft, particularily in a continuous kiln, where the temperature of the waste gases is very low.

The draft for burning is produced by an exhaust fan driven by an electric motor, the whole being mounted on a portable cover, constructed of I beams and sheet iron covering, as in the case of the drier covers. A suitable iron pipe connects the inlet of the fan with holes through the cover at the proper points. The draft cover is transported by the crane and brick lift, which engages a lifting beam, as will be seen by reference to Plate 17. The draft cover is deposited over a kiln chamber a suitable distance ahead of the fire, and is advanced from time to time as the fire progresses. Thus the source of draft is always at a fixed distance from the fire, is direct, positive and uniform, and at once effectually overcomes all the difficulties experienced with natural draft.

The kiln is built in a straight line instead of in the usual oval shape, in order to bring all the chambers properly within the range of the overhead crane, and it may be

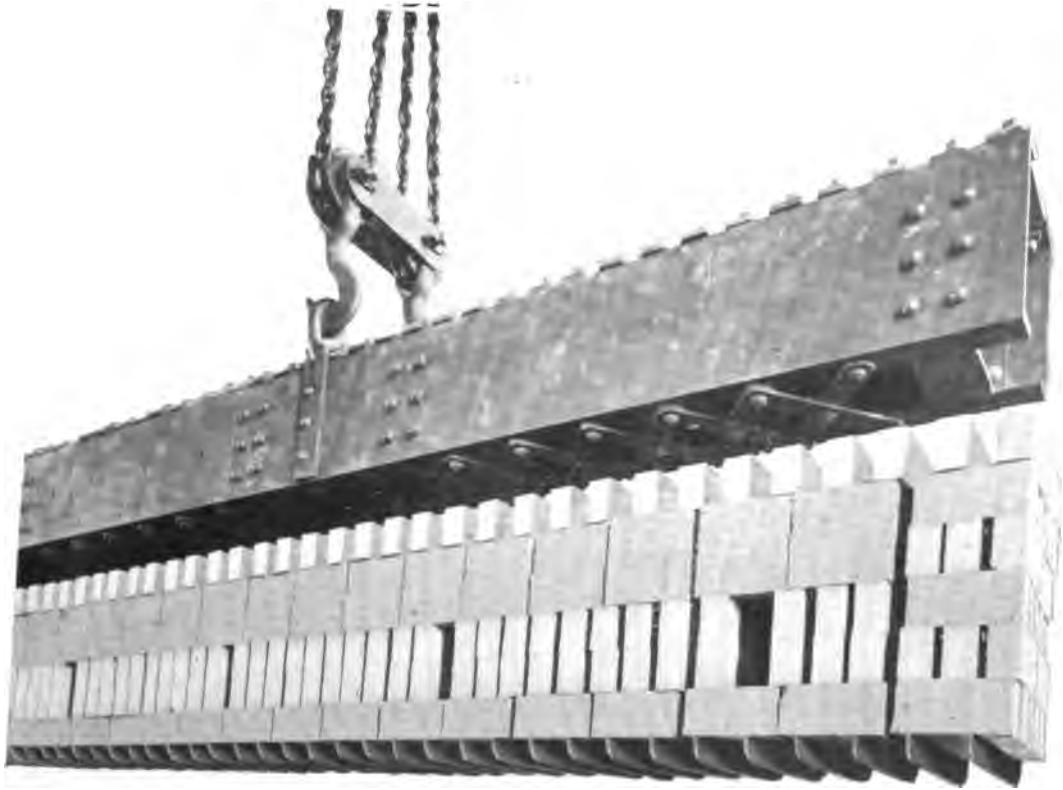


PLATE 7.—BRICK LIFT SUPPORTING A "UNIT STACK" OF BRICKS, SHOWING ONE END ONLY.

constructed on the semi-continuous principle, in which the fire is started at one end, traverses the length of the kiln and then dies out, or by the use of "return flues" connecting the two ends of the kiln, heat from the last chamber may be carried to the starting end for water smoking purposes, as in the case of the original continuous kiln. The latter arrangement is used in the Dover Point plant.

The improved continuous kiln not only saves two-thirds of the fuel and a large part of the labor of the up-draft wood burning kiln, but produces bricks of far more uniform color with practically no cracking, breakage or other loss.

The coal for firing the kiln is brought by the crane and brick lift, from the storage bin in long iron boxes, which are deposited on top of the kiln adjacent to the proper feed holes. These boxes are moved along from chamber to chamber as the fire progresses. Thus the wheeling of coal is minimized and no coal is wasted by being scattered over the top of the kiln. The ashes are taken out of the kiln by a similar box, which is lowered into the chamber after the bricks are removed.

When a chamber of burned bricks has become sufficiently cooled for removal to the stock pile, the crown is lifted by the crane and brick lift and deposited over a chamber just filled with dried brick. The brick lift is then lowered into the combustion space to a proper level, the fingers are run under the stacks of burned bricks, which are lifted and transported one by one to any desired point for assorting, storage and shipment. In plants making common bricks only the bricks may be deposited upon an automatic loading machine, consisting of an endless belt with separated slats or bars for receiving the bricks as in the case of the drier racks, this belt conveying the bricks and dumping them into the delivery wagon, thus reducing the entire hand labor on the plant to the single operation of stacking the soft bricks at the setting-up stands. Each unit stack will fill an ordinary delivery wagon. The same device may be used to load railway cars.

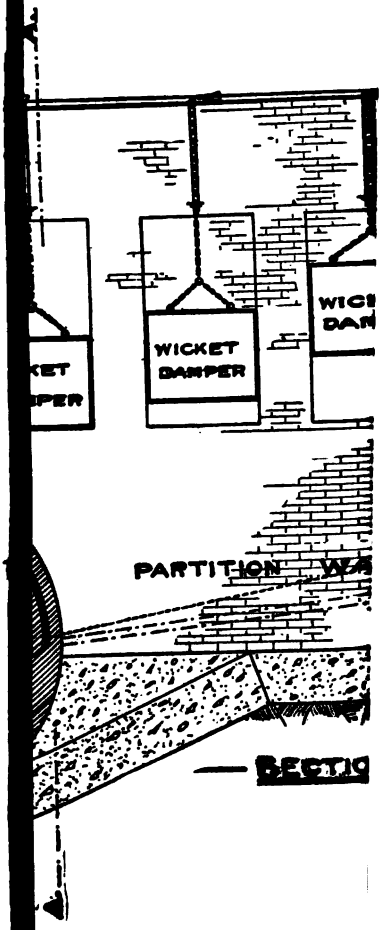
In the case of the Dover Point plant, some high grade

bricks are obtained by assorting, and consequently a second hand operation is here introduced, the burned bricks being deposited upon an assorting stand at the end of the kiln.

In the plant as thus far constructed no labor-saving appliances are used in connection with storing or loading the bricks, but it is the intention of the company to increase the main building to about twice its present length and to construct more continuous kilns similar to the one now in use. A storage building will then be constructed at right angles to the present main building, and an electric overhead crane arranged to take the bricks in large loads after assorting and to place them in the storage building, later to be shipped either by rail or water to the distant market. The elevated crane track in the storage building will be extended to include the railroad tracks at one end and the shipping dock at the other, and the assorted bricks will be handled on cradles, each containing a cart load. The cars or shipping barges may then be run under a crane at the point of destination and the cradles of bricks lifted as a unit and placed in the delivery wagons, thus eliminating the usual expense of hand labor for loading. By this system it is therefore possible to manufacture common bricks and deliver them to the customer with but two manual handlings from the clay bank to the mason, one after molding and one after burning, thus reducing the total labor to a fraction of its present amount even in the most modern yards.

The plant under discussion has so recently been put into operation that I am unfortunately unable to present any data as to the cost of manufacture, but it will doubtless be sufficient at this time to say that the practicability of the entire scheme has been fully demonstrated and that no great difficulties have been encountered. At a later date I hope to have the pleasure of presenting some data as to the cost of handling by this system and of burning in the continuous kiln with portable mechanical draft.

The preparations of the drawing and the erection of the plant has been under the immediate supervision of the Consulting Engineer of the Company, Mr. John C. Ostrup, member Am. Soc. C. E., well known throughout the country from



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his connection with the Chicago elevated roads and as Designing Engineer of the Boston elevated railroad.

DISCUSSION.

Professor Edward Orton, Jr.: Inasmuch as several months have elapsed since the publication of this paper,* and Mr. Fiske has been running his plant in the meantime and thus gaining new experience, I would ask him whether he has any statements which he wishes to add to those which have been already published on this paper? If so, it would be proper to make them now.

Mr. J. Parker B. Fiske: The secretary has been corresponding with me quite insistently for several weeks in regard to furnishing some figures in connection with the cost of making brick at our plant by the new method. I have told him that our plant has been put into operation so near to the time of this meeting that we are not able to present any figures which I really think would do justice to the plant, as it stands today. We have no records which represent the operation of the plant over any considerable period of regular manufacture.

I presume it is hardly necessary to say to those who visited the plant and saw what we are doing, that a plant as novel as this, and representing practically a new conception of brick making, could hardly be expected to be put into steady, systematic operation within two or three months after the construction was completed. As a matter of fact, we commenced regular brick-making operations about the first of November. We have been through the usual category of troubles incident to starting a new plant. I think I am right in saying that in starting a manufacturing plant of any kind, even of the most ordinary nature, that difficulties of some sort or other are almost inevitable for a time until the plant is brought down to a good working basis.

We are just emerging from the development or experimental period in our plant. For instance, we have burned

* NOTE BY SECRETARY: This paper was printed and distributed to the members of the society as advance sheets on December 10, 1902, and discussion at the Boston meeting specially invited. Also, the members visited the plant in a body on Tuesday afternoon, February 3, 1903, the day before this discussion was held.

our continuous kiln only four times. The first time was practically the drying out or burning out of the kiln. That suggested some changes which have been made, coincident with the further burning of the kiln, and they have been made, of course, at the expense of rapid and economical burning, because the masons naturally interfered more or less with the burning.

So far as the handling of the brick is concerned, we have proceeded far enough with that to feel pretty sure of what we can accomplish. I may say that the figures, in a general way, will be as follows: With the size of unit stack we are now handling, fifteen hundred brick, and with the speed of hoisting and travel to which our crane is now adjusted, we find we can handle fifteen hundred brick from one point to any other point in the factory, making the round trip in about five minutes. That requires the services of a crane man, part of the time of a helper, riding on the brick lift. On that basis we have repeatedly set a chamber of 18,000 brick in the continuous kiln, with two men, in one hour's time. That is at the rate of 180,000 brick handled into the kiln in ten hours by two men, or at the rate of 90,000 handled by one man in ten hours.

I think that a fair statement would be that a kiln setter, with three or four wheelers from an open yard, will handle 20,000 brick in about ten hours' time: four men, 20,000 brick in ten hours, or, one man, 5,000 brick in ten hours, as against one man handling 90,000 in ten hours by our method. That is a ratio of efficiency of 18 to 1.

Now, there is, of course, a large amount of other work which the crane has to do. It must handle the kiln covers, drier racks, drier covers, etc., all of which somewhat reduces its capacity for handling brick.

In regard to cost of operating the drier and kiln, my previous remarks will hold true, and we cannot give any figures which we think would represent what we will ultimately be able to do. I have no objection to telling people what we are doing, however, and I will say that we are operating our Crown Heat Generator with about 150 pounds of coal for each thousand brick dried. That, I feel certain,

PLATE 10.—LOOKING DOWN INTO DRYER CHAMBER, THE COVER AND TWO PORTABLE RACKS BEING REMOVED. TWO STACKS OF SOFT BRICKS ARE SHOWN, PLACED IN POSITION FOR DRYING.

does not represent the ultimate economy. The designers and builders of the heat generator guaranteed, with one pound of water in each brick, to dry 1,000 brick with 100 pounds of coal, which, of course, is a small amount of fuel. The way in which the drier does its work is not very different from the ordinary progressive drier, where the source of heat is fixed and the ware is progressed through to the zone of maximum heat. In our case, it is impossible to progress the bricks, so the zone of maximum heat is progressed through the bricks. Thus, we get the same progression of temperature on the bricks in our drier which is found necessary in other driers. The hot, fresh air is not turned directly on the soft, green brick, but turned first on the brick which are nearly dry, progressing forward to the bricks which have been freshly put in the drier.

As I said, we have not yet operated the kiln under favorable conditions, but I have a report which was handed me yesterday on one of the chambers which has just been finished, on the fifth burn. The amount of coal burned in that chamber was 6,630 pounds, the chamber containing 18,000 brick. That is about three hundred and sixty-odd pounds of coal per thousand brick. I feel very sure that this will be reduced to at most three hundred pounds, but am not able to give further figures as yet.

I think that is all I have to say now, but I will be glad to answer any questions which any member may desire to ask.

Mr. Willard D. Richardson: The fact that this paper has been before us for several weeks and also the fact that my name has been upon this program as one of the persons expected to discuss it, would preclude any plea of insufficient time for preparation. Before leaving for this convention, I had the excuse that I had not seen the plant and so left preparation for discussion until after the trip to Dover Point; and while it was possible to have made preparation since visiting the plant yesterday, it has not been practicable.

I am glad, however, of this opportunity to express my appreciation of the work which has been done by Mr. Fiske at the plant at Dover and I feel the whole brick-making industry is indebted to Mr. Fiske for the large experiments he

has carried on there. Whether it will result in the repetition of such a plant in other places on substantially the same lines or not, the work he has done will lead to a great advance, I believe, in the economical manufacture of common brick. It will stimulate other efforts, perhaps along other lines, to accomplish what Mr. Fiske has been endeavoring to accomplish; that is, to eliminate hand labor in brick making.

After looking over the plant yesterday, I attempted to make some figures myself, but I found I could not make any figures which would be at all satisfactory as to the saving that might be accomplished by this system, and I can easily understand why Mr. Fiske cannot give any figures which would be of any value as to the saving actually being made at the present time. He knows, however, better than we can, how much his plant has cost above that of a modern plant of equal capacity having the car system, tunnel driers and continuous kilns; also whether the exhibition made to us yesterday represented the average daily operation of the plant, whether the chambers we saw open showed exceptionally good results or otherwise, whether the burned brick can always be picked up from the chambers as easily as we saw it done; whether the crane has already caused expense for repairs and whether the kiln covers are expensive to keep up, etc.

I have come up against this same problem of reducing hand-labor and studied a great deal merely upon the question of how to do away with the wheelbarrow—the hard labor of the wheelbarrow. Several times I have studied out and designed conveyors which would do away with wheelbarrows. My plans looked all right and I have been on the point of applying them to the carrying of clay and brick. But after making accurate calculations I always came back to the same conclusion, that upon the scale of operations which I have been conducting, while I could easily eliminate the hard work of the wheelbarrow, I could not save any money by it. I could not save a dollar by it, and so long as that labor is to be had, so long as men wanted to wheel the barrow every day, I continued to use the barrow.

PLATE II.—CRANE HANDLING PORTABLE DRYER RACK.

So in Mr. Fiske's system, in order to show a saving of money by the saving of hand labor, the operations must be conducted on a large scale. When he has his plans completely carried out at Dover Point, he will probably be able to show a large saving and a profitable business, but I cannot figure such to be the case in the small experimental plant he has now in operation—small, not in most of its appointments nor in its fixed expenses, but in its daily output.

Mr. Ellis Lovejoy: There is a great deal of credit due Mr. Fiske for the work which he has put into the development of this process. I had some talks with him while he was working it up, and I must confess that through it all I thought he was on rather a wild goose chase. I want to say to him now, that seeing the plant in operation yesterday was quite a surprise to me. I did not expect to see the work carried on as smoothly and with as little difficulty as was done. I am frank to admit that he can handle his unit of fifteen hundred brick from the machine to the dry house, from the dry house to the kiln, and with the clay he is burning, from the kiln to the stockyard. There are some clays, however, which would give him trouble. If, however, the work is limited to common brick, made from clay which will not stick, the process can be carried through from beginning to end.

What improvements he can make to take in all other lines is a question for the future. But I am glad to know he has succeeded in working out the mechanical difficulties which to me, in the beginning, appeared serious. The question with me all along has been the possible power of the crane. We timed it a number of times yesterday and found it would make the trip in from three to five minntes, and I am willing to concede to Mr. Fiske that it could do the work continuously in that time. In Mr. Fiske's figures, he gives it as within its power to handle 180,000 bricks. He means, I think, green bricks from the machine to the dry house; the second handling to the kiln reduces the quantity to 90,000; and the third handling, from the kiln to the finished product, including dead work, reduces it to 45,000. I think this is the figure which Mr. Fiske gave me as the capacity of the

crane per day—45,000 finished brick. Notwithstanding the number of trips the machine would have to make, bringing in the coal, changing covers, moving racks and removing ashes, I am satisfied that it could handle this product of 45,000 bricks in one day. The whole value, now, of this process, hinges on that one point. If 45,000 is the limit of his crane, then there can be no increase in the system without introducing another crane.

Now, there is no saving up to the point where his bricks are put upon the racks. In that, I think, all will agree. He has two men with the crane to take the 45,000 brick to the kiln. Two men, with properly constructed trucks, will move the same quantity on cars through the dry house to the kiln. So, as I look at it, there is no saving whatever up to the point where the bricks reach the kiln. At that point in the old method, we must introduce setters and tossers, and just there I am free to admit I am a little lame. I am not familiar enough with common-brick business to know just what is required for a days' work in setting. I have heard of instances where a single setter put over fifty thousand bricks in a kiln in one day. I think it is common practice to put at least thirty thousand. But let us assume that he puts in twenty-two and a half thousand. Then it will take two setters to take care of the bricks that this crane would handle. It will take two or three tossers to deliver these bricks to the setter. If I am wrong in my estimates, I hope those who are familiar with the subject will correct me. The two setters and three tossers would cost about nine dollars a day, depending on the rate of wages, which would amount to twenty-seven or twenty-eight hundred dollars in the course of the year.

To offset this, Mr. Fiske has introduced an expensive piece of machinery, the crane itself. I have not at hand any figures giving the cost of his crane or the building to carry it, but there is a considerable expense in the building and the crane over the cost of an ordinary yard kiln shed. If this excess of cost amounts to ten thousand dollars, then he has an investment of that amount, and that investment in business is worth from eight hundred to one thousand dollars

PLATE 12.—SHOWING HOW THE BRICK LIFT ENGAGES THE PORTABLE DRYER RACK.

PLATE 14 — BRICK LIFT DEPOSITING A STACK OF BRICKS ON KILN FLOOR BLOCKS; THIS WAS TAKEN DURING THE CONSTRUCTION OF KILN, BEFORE PARTITION WALLS WERE BUILT.

per year. If the excess is twenty thousand dollars, then he has money invested which represents an interest value in a factory of from sixteen hundred to two thousand dollars. If he has an excess of thirty thousand dollars invested, then he has interest cost exceeding the cost of labor to set the bricks. The difference in saving at that point hinges entirely upon the additional expense of his plant.

From that point on, his crane will move the brick to the sorting table. At the sorting table, as he is working at the present time, the bricks are rehandled, and in the plant as at present operated, there is no saving at that point, because the men who rehandle the brick at the end of the process could quite as easily take them from the kiln. We admit that he may have a saving up to the time the bricks are set in the kiln, of from sixteen hundred to two thousand dollars per year. As the plant is at present operated, there is no saving beyond that point, but if he introduces his system at the finished end of his product, and, by the use of cranes instead of wheelbarrows, can handle his product and save the wheelers, there is a possibility,—a great possibility,—of more than doubling the saving he would make in setting the green bricks. In view of the unfinished state of his plant, we must give Mr. Fiske the benefit of this possibility.

As the plant is at present constructed, it has a machine capacity of sixty thousand, a crane capacity of forty-five thousand, and a kiln capacity of eighteen thousand brick per day. It may be possible for him, after a little experience with the kiln, to increase the rate at which he is now burning, and turn out the equivalent of a chamber and a half per day, which would bring it up to a little over one-half the capacity of his crane. I think he can do this. Now, then, to get his unit of 45,000 bricks,—the full capacity of his crane,—he must radically change the structure of his kiln. I do not believe Mr. Fiske is satisfied with the kiln part of his plant, and, no doubt, he already has ideas, based on his experience with this kiln, which would greatly improve that part of his factory. He cannot lengthen it without lowering the capacity of his crane. The thought was suggested by Mr. Richardson that he can put another kiln on the other

side of his machine, and this would not increase the distance his crane would have to travel; but that brings into the question the handling of the burned product at two points, and would mean the introduction of two cranes, so it seems to me that it is not to be considered.

I am not prepared to give any expression of opinion regarding the life of his kiln as compared with that of other continuous kilns, nor am I prepared to give any comparison of cost between his kiln and other continuous kilns; but those items of cost, if greater than that of other continuous kilns, increases the amount of money invested in the business and lowers the difference between the cost by this process and the older method.

He undoubtedly makes a great saving in fuel over any other system using common kilns, either up or down draft; but I do not think Mr. Fiske will claim any unusual saving in the use of his kiln over other types of the continuous kiln.

So the whole matter hinges upon the value of the crane in replacing the labor of the old method.

Mr. Fiske: In answer to some points made by Mr. Lovejoy, I will say that the plant, as it stands today, is of course not finished; and for that reason we do not claim to be making anything like the saving over the usual methods which we expect to make when the original conception of the plant is carried out. In plants making common brick only, which are the kind of plants where I think the greatest saving would be made, there is no sorting of bricks to be made at the finishing end of the plant. In that case, the bricks would be brought directly from the kiln by means of the crane, and either loaded directly on the cars by the crane, or put upon a loading belt which has cleats or bars across it, as in case of the drier racks, moving forward like the ordinary conveyor belt and loading into cars outside of the building.

I want also to say that the crane as we now have it in operation is not doing anywhere near its maximum work. I understand it is the practice in the large steel mills to have a speed of travel on the track of six hundred feet in a minute. Our speed is somewhat less than two hundred feet

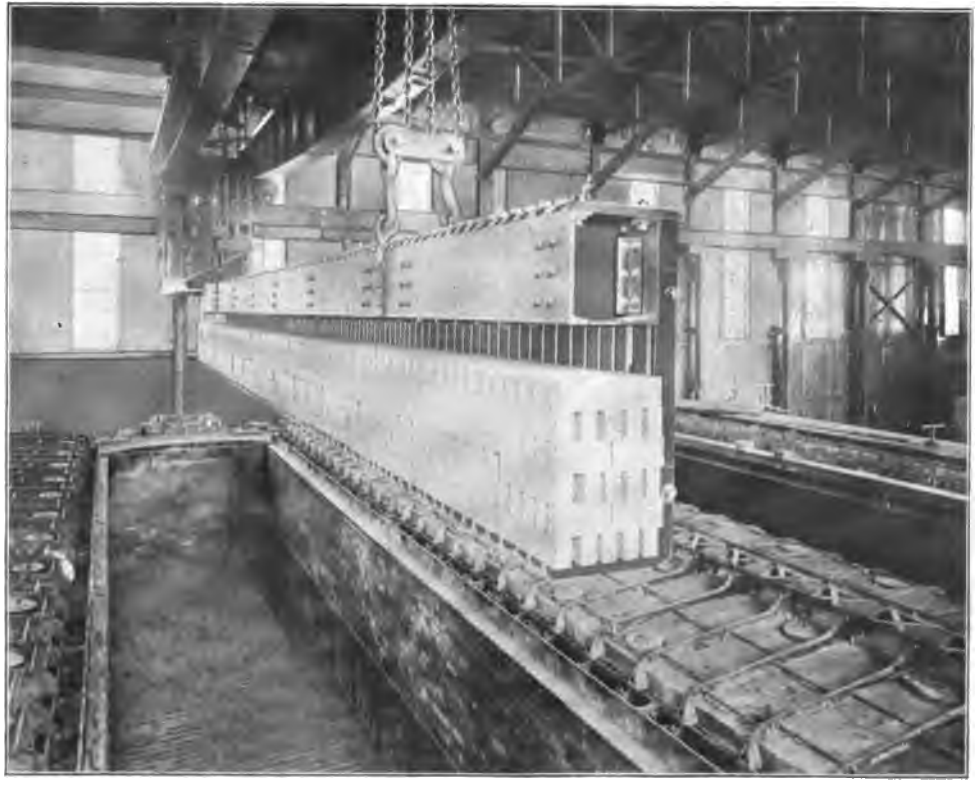


PLATE 13.—KILN CHAMBER WITH CROWN REMOVED, AND THE CRANE WITH STACK OF DRIED BRICKS READY FOR DEPOSIT IN THE KILN.

a minute at the present time. We have it so arranged that the crane may be speeded up later when necessity requires. The speed in hoisting can be in the neighborhood of twenty feet a minute for a load similar to the one which we carry. Our speed is some five or six feet a minute. So the value of the crane in this industry cannot be judged by the amount of material which we are actually handling today at Dover Point. It is a well known fact that the electric traveling crane, as much as any other one thing, has revolutionized the steel business, and the electric traveling crane has been found to be, of all the different devices so far invented, the most efficient for handling large masses of heavy material.

I do not wish to repeat things which I have said in my paper already presented to the society, but I do want to say this: that looking over the different manufacturing fields, it has been my belief for a long time that no field required the introduction of machinery for handling its product cheaply more than the brick business; because bricks, of all finished products in the world, are worth the least per ton of anything I know.

In regard to the cost of the crane and additional investment, I will say that our crane cost $4,200. That does not include the tracks, which I think cost some three or four hundred dollars. Five thousand dollars, as an outside figure, will represent the installation of the crane. It must be borne in mind, however, (or, I will have to ask you for the moment to take it for granted) that this crane will handle, say fifteen million brick per year. For handling any such product as that by the car system and tunnel driers, a large investment in tracks, turn-tables, cars, etc., must be made. That would go a long ways to offset the cost of this crane.

Moreover, the item of repairs and depreciation on the traveling crane is a very small factor; while we know that the cost of repairs and depreciation on the car system is a large factor and a very troublesome one. There is hardly a large plant today but has more or less of its cars in the repair shop. The extra cost of the building should not be charged to the crane, or to this system, because while it is true that we have a more expensive building than is put up on the

ordinary brick yard, we have a building which is more permanent, which will represent less cost for repairs and less depreciation, and is in the nature of an improvement itself independent of its connection with the crane. It is in the line of better construction, which I believe we are all coming to in the ceramic industries.

In regard to the capacity of the crane, as operated today, Mr. Lovejoy's figures are about correct. That is to say, we figure that the crane will do, on straight handling work, about 180,000 brick in ten hours for one movement of the bricks. As we have three movements, the number of brick will be reduced to one-third, or sixty thousand; and making liberal allowance for the time in taking the brick out of the kiln, handling covers, ashes, coal, etc., I think forty-five thousand is a fair estimate. In plants representing large investments, however, it is the custom to operate them more than ten hours a day; and the larger your investment, the more profitable it is to operate your plant overtime. It is our intention, when occasion requires it, to have this crane do a portion of its work at night. We have electric lights, and can see to do our work as well by night as by day; so the capacity of the single crane can easily be increased to sixty thousand brick in twenty-four hours.

The cost of the kiln has been touched upon, and I will say that no extra expense, over and above the ordinary kiln, is necessary by reason of this system. In fact, while some items are somewhat more expensive, as the kiln cover, the construction of the balance of the kiln is somewhat cheaper owing to the absence of doorways, complicated flue system, damper system, etc. There are many general advantages which we see in this system over the ordinary continuous kiln set by hand, and which operators of continuous kilns have appreciated. One is the ability to uncover the burned bricks when the burn is completed, by taking off the entire cover from the kiln and thus allow it to cool quickly, and allowing plenty of light and air for the men necessary to work in the kiln. One great disadvantage of the continuous kiln today is the fact that the men have to work in the hot, dark kiln chamber, isolated from the rest of the operators,

PLATE 15.—KILN WITH CHAMBER OF GREEN BRICK IN THE FOREGROUND READY FOR RECEPTION OF KILN COVER.

and not directly under the eye of the superintendent; and their work probably costs more than it would in other parts of the plant. In case of the kiln with removable covers, there is a better place to work and the men are immediately under the eye of the superintendent.

With the increase of speed which is perfectly practical in connection with the crane, there will be no difficulty, so far as we have been able to judge by our experience so far, in providing extra kiln capacity by lengthening the kiln in the present direction, which is in accordance with our original plans. It would not be practical to build another kiln at the other end, because of the difficulties which Mr. Lovejoy mentioned.

Prof. H. A. Wheeler: I think Mr. Fiske is entitled to the highest compliment for the bold,courageous, and very successful way in which he has started on this new trail; and I think it was unfortunate that we were so kindly invited to go out to inspect the plant when there had been such a limited time in which to get it into smooth, working condition, which requires such a long time in any industry. We are all aware of the fact that before any concern becomes money-making, besides the first investment of capital, and the investment of capital to carry on the business, the working capital, there is another investment necessary which sometimes exceeds the other two, and that is for effective organization. It is something which even engineers often overlook entirely, or at least largely neglect to take into account, and which very few capitalists ever realize the importance of. The building up of an effective organization is something which is seldom done under one year, and often it requires several years. So I want to express my hearty appreciation, not only of Mr. Fiske's courage, but of the smooth, successful way he has got the plant in operation in the very limited time since the construction was completed. While I very much enjoyed Mr. Lovejoy's criticisms, I must say that Mr. Fiske is blazing a trail which is going to prove of much greater importance in the future than now appears. Then there is increased insurance rate, extra cost of wear and tear, and similar items which Mr. Lovejoy has overlooked as pertaining to the old

method, which will materially increase the comparative saving.

Its elimination of a large number of laborers will in the future make it valuable. The labor problem is becoming a very serious one in this country, and anything which tends to eliminate the slavery of the walking delegate which is going to enable us to carry on our business as we may wish, free from the control of the labor master, is going to mean the differences of success and failure. That thing alone, in the future, is likely to become much more important than any mere slight saving above the cost by another method. The recent anthracite strike is a mere rumbling of the battle ahead. We can always control a few men. In any well managed organization, if the foreman has any tact and diplomacy, there are always a few reliable men who may be depended upon. But when it comes to the control of masses of these laboring men, we are up against a serious question. As the labor organizations become more and more powerful, with leaders in some instances of very high ability and generalship, the question becomes more serious, and anything largely reducing labor perils, enabling us to get along with fewer and fewer men, is going to be the only successful way for handling the labor problem in the future. The great engineering strike in England is what really put this nation where it is today in its industrial condition and gave Germany the enormous advantage which it now holds, putting England back to third place where it was unquestionably first. It was the walking delegate who caused England to drop behind to third place, and it is the walking delegate today who is rapidly making America's industrial condition more and more unsafe, and making capital more and more conservative as to engaging in manufacturing enterprises.

Professor Orton: I am not going to attempt to discuss the merits of Mr. Fiske's plant, but there are statements of Mr. Richardson and Professor Wheeler which I can not allow to pass undisputed. I do not believe that any permanent good is going to come to the human race by an attempt to accomplish by mechanical effort what is now done

PLATE 16.—REMOVABLE KILN CROWN BEING HANDLED BY CRANE AND LIFT.

by hand labor. Mankind is here and must have a living. Men must have a certain amount of physical exercise, a certain amount of work. Any way which provides mechanical means of making money, without depriving man of the opportunity for work, is to be commended; but to actually supplant labor by machinery seems to me a superficial way of treating the labor problem. You cannot perform work mechanically except by the combustion of fuel. Of course, there is a moderate supply of water power which we may use more in the future, and we may use wind power more than now. But as things are now constituted, the bulk of our power comes from combustion of fuel. The heritage of fuel which we have is something which is not growing and increasing. The fuel which we are now using is largely stored or fossil fuel. Mr. Richardson has stated that wherever it is possible to do a thing mechanically at the same cost that it can be done by labor, it should be done by the mechanical method. I would take exactly the reverse stand. I would do by hand labor, everything which can be done best that way, and only do by machinery that which the good of the human race requires to be done in that way. If in one case a great economy results from mechanical methods, so that an industrial product can be turned out at a less cost, and thus reach more people, and provide more comfort to more people, then let the mechanical means be employed. But if the cost of the two methods is just the same, then I say give labor the work. Instead of attempting to get rid of the slavery of the walking delegate by substituting mechanical means for hand labor, rather try to get closer to our brothers of the laboring class, and develop in them and in ourselves humanitarian principles and sane coöperation, by which alone a permanent improvement can be made.

Mr. W. D. Richardson: I desire to correct a wrong impression which may have been made by what I said awhile ago regarding hand labor. If a man can make improvements in brick manufacture which will eliminate hand labor, even if not effecting a great saving, I believe it ought to be done. It is something which ought to be done in

manufacturing lines all the way through. If this improvement lessens labor and can be operated without greater expense to the company than the employment of hand labor, including of course, interest, repairs and depreciation, it ought to be adopted.

I base this statement upon humanitarian as well as economical grounds, not with any idea that the elimination of hand labor is going to solve the labor question, only insomuch as the accomplishment of laborious operations by mechanical means tends to elevate any industry and to place all who labor upon a greater equality. We cannot eliminate the laborer, and the labor question must continue to demand wise and careful consideration. But to increase the production per man, in any line of work, and at the same decrease the hard manual labor involved, cannot but be moving in the right direction and lead to a shortening of the hours of labor with greater comforts to all. Let no one fear labor-saving inventions. The more that can be accomplished in any line with a given amount of labor the greater opportunity, for development in other lines and the better for the future welfare of the people of the whole world.

That is why I am especially interested in this attempt of Mr. Fiske's to save the hard hand labor in brick manufacture. Mr. Lovejoy has gone into the question very thoroughly—I knew he would—and we have a very fair estimate of what may be expected from the new system which Mr. Fiske has developed. Of course, in the figures which Mr. Fiske has given us in regard to the drier, kiln, etc., there is nothing especially pertaining to the Fiske system. The cost of drying brick has not been greater by the car system, nor is the cost of burning greater in other continuous kilns. One point I thought of yesterday which I want also to bring out, and that is the fact that the success of his system is not entirely dependent use of his continuous kiln. Any open top continuous kiln of sufficient width could be used, or, the labor of handling the brick could be saved by setting in the same manner in an updraft kiln. Of course, such up-draft kiln must have permanent arches under a permanent perforated floor. He could set a large kiln of brick of several

PLATE 19.—SHOWING PORTABLE DRAFT APPARATUS, IN POSITION ON THE LAST CHAMBER OF THE KILN.

hundred thousand and still have a saving in the labor of setting the brick.

At this point, however, we come to a stop in our calculations, for the difficulties of this system now begin, and we are in doubt about their being entirely overcome. There is nothing impracticable in handling 1500 or more brick with a crane, picking them up and setting them down wherever desired along the line of its travel. So that there is need of no further demonstration after what we saw yesterday to prove that brick can be set by this method cheaper than by hand. The greater problem, which I am unable to say yet has been successfully solved, is to construct a continuous kiln that will permit of being set in this manner and give good burns; or to construct any kiln in which brick from ordinary clays can be burned to a good degree of hardness and the brick so evenly spaced in their original position that they can be picked up again without serious difficulty by the fingers of the crane. When it has been proven that this can be done the system will be a success and will be sought after by others, and become an important factor in the brick business. I shall watch the solution of this problem with a with a great deal of interest. It is up to Mr. Fiske, and I wish him all success.

THE DEVELOPMENT OF THE "MATT" GLAZE.

BY

CHARLES F. BINNS, M. SC.

Some forty years ago, Mr. R. W. Binns, my late honored father, then the Director of the Royal Worcester Porcelain Works, conceived the idea of covering the brilliant surface of a porcelain glaze with a semi-lustrous or texture color. The intent was to check the strong reflections of a shining glaze and to impart a soft sheen which would combine harmoniously with furniture and drapery. The specimens executed were shown to certain buyers who refused to acknowledge in them any merit whatever. Nothing daunted, the originator set the work aside, saying that some day it would be sought after.

The prophecy was fulfilled about fifteen years later. Dead surface ware was demanded and produced with an unparalleled success which lives in the memory of many, and which resulted in the term "Royal Worcester" being regarded as synonymous with dead or matt colors.

The idea of dull surface was taken up by artists, to whom it specially appealed. Architects specified dead glaze for interior tile work as well as terra cotta, and manufacturers all over the world sought to supply the need.

A variety of expedients were resorted to in order to produce the required dullness. The attack of fluoric vapors, abrasion by sand-blast, an immature glaze, have all been tried, and it is not to be wondered at that manufacturers who have had no other resource have abandoned the pursuit in despair.

When I began to investigate the problem, the thought of devitrification was present. It is well known that a highly acid glaze, low in alumina, will, if cooled slowly, lose its gloss, presenting a surface more or less dead and sometimes

even iridescent. But experiments embodying this idea did not succeed, probably because an attempt was made to burn the glazes in a general kiln and the cooling was not slow enough.

The matter stood at this point when the date of our last annual meeting came around. On that occasion Francis W. Walker presented his paper on copper greens. Among his numerous examples there was one which started a train of thought the end of which is not yet.

This glaze differed in appearance from others in the same series, by reason of the fact that it possessed a delicate texture surface. Not merely dull but soft and silky. The touch brought back vividly the memory of the Worcester ivory, and perhaps this helped to deepen the impression. Turning to the formula of the glaze in question it was found that the essential difference from its fellows lay in the content of Al_2O_3. This stood at .35 and the figure opened a new world.

In passing, one may point out that this fact is a remarkable illustration of the value of thinking in formulæ. By no other possible means could it be known that the figure was at all unusual at the heat used or would it have conveyed any idea.

That night the matter was turned over and pondered, even to the "wee sma' 'oors" — "Alumina 0.35" repeated itself over and over until the resolve was taken that the point should be threshed out.

Several questions presented themselves—

1. Does the alumina content govern the production of texture?

2. Will the same alumina content produce the same result with different bases?

3. How far will the acid content affect the result?

4. To what physical or chemical cause is the texture due?

As a means of answering these questions the following base combinations were arranged:

1	PbO	0.7	2	PbO	0.7
	CaO	0.3		CaO	0.2
				ZnO	0.1
3	PbO	0.6	4	PbO	0.55
	CaO	0.2		CaO	0.30
	ZnO	0.2		K_2O	0.15
5	PbO	0.5	6	PbO	0.5
	CaO	0.3		CaO	0.2
	ZnO	0.2		ZnO	0.2
				K_2O	0.1
7	PbO	0.5	8	PbO	0.5
	BaO	0.2		CaO	0.2
	ZnO	0.2		BaO	0.2
	K_2O	0.1		K_2O	0.1
9	PbO	0.5	10	PbO	0.5
	CaO	0.2		CaO	0.1
	BaO	0.2		BaO	0.1
	ZnO	0.1		ZnO	0.2
				K_2O	0.1

Each of these bases was made up in two extremes, one having alumina 0.25 and the other alumina 0.50, the content of silica being maintained at 1.60, the amount in the Walker glaze.

All the mixtures were made as raw glazes and ground in a ball-mill. It will at once be seen that the idea here was to find the content of alumina which produced the best texture with each base. The specimens shown consist of the two extremes and the best intermediate in each series. The composition of each mix is given. The extremes were weighed up and ground. They were then dried and the proportionate equivalents weighed out. Mucilage was freely used in order to secure a thick body of glaze, and the trial pieces consisted of tile furnished by the Beaver Falls Art Tile Co. All except the first series were fired flat, the heat throughout being cone 01.

It is due partly to the nature of the body and partly to the horizontal position that so many of the glazes have tucked and parted. Similar glazes upon upright surfaces

and plastic bodies have been entirely free from this trouble, but it may be mentioned here that the difficulty almost always occurs in drying. If a matt glaze be cracked, or at all imperfect on entering the kiln, it will be unable to correct the fault. But if it be sound when dry, it will in all probability remain sound when burned.

The following table indicates the appearance of each glaze and the content of alumina at which the best surface was found, not always good, of course, but the best of its series:

No.	Lower Extreme Al_2O_3 0.25	Best Intermediate	Higher Extreme Al_2O_3 0.50
1	Semi bright	Slight scum, harsh Al_2O_3 0.375	Parted
2	Nearly bright	Dull matt, parted Al_2O_3 0.375	Parted, dead
3	Bright, scummed	Good matt, parted Al_2O_3 0.375	Quite dead
4	Bright, immature	Good matt, but scummed Al_2O_3 0.35	Quite dead
5	Bright, immature	Dead matt Al_2O_3 0.375	Quite dead
6	Bright but sluggish	Good matt, slight scum Al_2O_3 0.375	Matt but parted
7	Dead	Dead matt, good face Al_2O_3 0.375	Parted, infusible
8	Fine matt	Dead matt Al_2O_3 0.275	Dead
9	Good matt	Fine matt Al_2O_3 0.325	Dead, parted
10	Scummed matt	Good matt Al_2O_3 0.35	Quite dead

Reviewing the above it will be seen that the more complicated bases give the best results, and that the alumina content for a good matt is always close to the figure 0.35, varying slightly with the fusibility of the base. The glazes being raw, barium must be considered as less fusible than calcium. Barium seems to contribute towards a fine silky

texture, but will not carry quite so much alumina as calcium.

Assuming from these results that Al_2O_3 0.35 was about the most favorable content, certain other points were pursued:

No. 11 Interchange of bases. Two glazes 11-1 and 11-4 were made up and blended. The RO's were

$$11\text{-}1 \left\{ \begin{array}{ll} PbO & 0.575 \\ CaO & 0.425 \end{array} \right. \quad \text{and} \quad 11\text{-}4 \left\{ \begin{array}{ll} PbO & 0.575 \\ K_2O & 0.225 \\ ZnO & 0.200 \end{array} \right.$$

while the alumina and silica were 0.35 and 1.60 respectively in both glazes.

Three examples from the series are shown:

No. 11-4 is a dead matt with a beautiful crystal developed in the surface, working the glaze into a "crocodile skin" texture.

No. 11-2 has the formula

$$\left. \begin{array}{ll} PbO & 0.575 \\ CaO & 0.170 \\ K_2O & 0.135 \\ ZnO & 0.120 \end{array} \right\} Al_2O_3\ 0.35 \quad SiO_2\ 1.60$$

and is a fine matt glaze.

No. 11-3 is also a good matt, but badly parted, due to drying cracks. The formula is:

$$\left. \begin{array}{ll} PbO & 0.5750 \\ CaO & 0.2125 \\ K_2O & 0.1125 \\ ZnO & 0.1000 \end{array} \right\} Al_2O_3\ 0.85 \quad SiO_2\ 1.60$$

Of series 12, compounded on similar lines, three specimens are given:

No. 12-1, an interesting crystal glaze, bright rather than matt, contains

$$\left. \begin{array}{ll} PbO & 0.575 \\ CaO & 0.200 \\ ZnO & 0.225 \end{array} \right\} Al_2O_3\ 0.35 \quad SiO_2\ 1.60$$

No. 12-2, a beautiful texture matt, has

$$\left. \begin{array}{ll} PbO & 0.575 \\ CaO & 0.200 \\ ZnO & 0.135 \\ K_2O & 0.090 \end{array} \right\} Al_2O_3\ 0.35 \quad SiO_2\ 1.60$$

and No. 12-3, a very fine matt, has

$$\left. \begin{array}{ll} PbO & 0.575 \\ CaO & 0.200 \\ K_2O & 0.225 \end{array} \right\} Al_2O_3\ 0.35 \quad SiO_2\ 1.60$$

The next point to present itself was the influence of silica upon a glaze which possessed the constitution of a matt.

To throw light upon this the following series was laid down:

$$\text{No. 13}\quad \left.\begin{array}{l} PbO\ 0.5 \\ CaO\ 0.2 \\ ZnO\ 0.1 \\ K_2O\ 0.1 \end{array}\right\} Al_2O_3\ 0.4 \left\{\begin{array}{l} SiO_2\ 2.5\ \text{(higher extreme)} \\ \quad\text{to} \\ SiO_2\ 1.2\ \text{(lower extreme)} \end{array}\right.$$

The latter of these gave a good matt as was expected, the former showed a distinct brightening of the surface and increased mobility, the significance of which will be pointed out later.

The best intermediate contained SiO_2 1.95.

The behavior of barium in the earlier trials had suggested that with this base a lower content of alumina would be advisable. A new series was therefore made up.

$$\text{No. 14}\quad \left.\begin{array}{l} PbO\ 0.3 \\ CaO\ 0.2 \\ BaO\ 0.3 \\ ZnO\ 0.2 \end{array}\right\} \begin{array}{l} Al_2O_3\ 0.40\ \text{(higher extreme)} \\ \quad\text{to} \\ Al_2O_3\ 0.15\ \text{(lower extreme)} \end{array} \left\{ SiO_2\ 1.6 \right.$$

These are shown, together with an intermediate carrying Al_2O_3 0.225. The last named is somewhat hard; the lower extreme is a good matt, but the higher is perfectly dry.

Yet another series had for its objective the interchange of barium and calcium:

No. 15-1 contains

$$\left.\begin{array}{l} PbO\ 0.4 \\ CaO\ 0.3 \\ ZnO\ 0.2 \\ K_2O\ 0.1 \end{array}\right\} Al_2O_3\ 0.35\ \ SiO_2\ 1.60$$

No. 15-2 has

$$\left.\begin{array}{l} PbO\ 0.4 \\ BaO\ 0.3 \\ ZnO\ 0.2 \\ K_2O\ 0.1 \end{array}\right\} Al_2O_3\ 0.35\ \ SiO_2\ 1.60$$

The difference is very marked. No. 15-1 is a good matt though parted from drying cracks, 15-2 is hopelessly harsh and dry, while the best intermediate, carrying CaO 0.18 and BaO 0.12 is scarcely fused to a texture.

Reverting now to the questions propounded at the beginning of this paper, the enquiry may be made as to whether satisfactory answers have been found.

The scope of the enquiry has been somewhat limited, in that the influence of alumina has been pursued almost to the exclusion of other points, perhaps equally important. That alumina has a very large part to play in the production of matt glazes is undoubted. So far as this enquiry has gone, it may be said to have the most important part of any glaze constituent. In fact, that a good texture matt can always be produced by excess of alumina. What "excess" means depends upon the base used on the one hand and upon the acid content on the other. Different base combinations require different contents of alumina, but within comparatively narrow limits. Lead seems indispensable but an excess is injurious. Zinc is very desirable as are also potash and lime. Barium would probably act differently if fritted. In the raw state it does not seem reliable, though it enables the alumina to be reduced in amount.

The influence of silica is instructive. The addition of even a small amount to a matt glaze will brighten the surface, pointing to the probability that the matt texture is due to particles of alumina which have been rejected by the glaze composition, but which are siezed upon by the extra modicum of silica and brought into combination. This point will throw light upon the last question—the physical or chemical cause of the matt texture. In the first place it is certain that it is not caused by flotation. This is the difference between texture and scum. A good texture glaze is the same throughout its substance, the rejected particles are not thrown to the surface. Again, the texture is not due to immaturity. Some of these glazes have been fired to cone 9 without losing their quality.

I believe that the matt is caused by a disarrangement of the chemical relationship which exists between glaze constituents under normal conditions. While on the one hand an excess of acid may afford a texture by devitrification, which is only a name for the appearance caused by a rejection and separation of particles of silica, so, on similar but

opposing lines, another and different texture may be caused by an excess and consequent rejection of basic molecules.

The stability of the fired glazes has not been touched upon in this investigation. Some texture glazes are subject to decomposition from atmospheric influence, and they may be tested with acid vapors, but in the experiment undertaken here this has not been done.

DISCUSSION.

Mr. E. C. Stover: Mr. Walker, I will ask you whether, in the series of trials you made last year, you put through a series without color, in the various glazes included in your oxide of copper tests?

Mr. Walker: I did not. I don't think the small percentage of copper in my experiments would have had any appreciable effect. I think it would be found that the glaze without the copper would be very much the same as with it. I judge that, from looking at Professor Binns' trials which have been handed around.

Mr. Thomas Gray: Three or four weeks ago I had some matt glazes which I did not want. It was brought about by firing very rapidly with soft coal, only using about twelve hours start to finish. About one-half the kiln came out fairly good, but the rest was quite matt.

Mr. Karl Langenbeck: It seems to me hard to discuss such a paper, because it deals with matters of fact along a particular line, but a small part of the line being covered. Professor Orton recently conducted a line of experiments with glasses, and I think he could possibly throw some light upon the question, whether in an amorphous silicate, if the relations of alumina to silica are fixed within certain limits, the excess of alumina is thrown out and remains in the glaze; or whether, on the other hand, the limits are exceeded in point of silica and whether the silica is thrown out in a crystalline state?

The other question which comes up is the one indicated by the president, upon which Mr. Walker's immediate experiments do not seem to throw much light; but Mr. Grueby has stated to me that in his experiments, the cupric oxide

undoubtedly interfered with this behavior. The question is whether other oxides are substitutable for alumina?

Edward Orton, Jr.: We all feel the impossibility of giving anything like an adequate discussion of a paper like this. It simply emphasizes the fact that if we are going to do high grade work in the way of discussing papers, we must have them printed and sent to the members *in advance*, so we may be prepared to discuss them. No one can discuss a piece of work as painstaking as this, without giving it some study.

Regarding the experiments with glass to which reference has been made, I would say that they do not suggest anything in this connection, except the one fact that alumina does not always seem to assist in the production of mattness; but, on the other hand, may frequently powerfully assist in the production of glassiness. That principle was first stated by Mr. Langenbeck in his book, when he called attention to the difference between glasses and glazes, i. e., that in the glass, the ware is cooled so suddenly that no chance for devitrification is given, and alumina is therefore not needed and is, in fact, either low in amount or altogether absent, while in the glaze, it is only by the use of alumina that we can prevent devitrification during the long cooling in the kiln. I do not remember having seen elsewhere in ceramic literature this distinction so sharply drawn as Langenbeck made it there. One of my glasses produced last year showed this point beautifully. It was a series of glasses in which I had been persistently reducing the silica contents. I started with RO, 2.5 SiO_2, and reduced the SiO_2 one-half equivalent at a time to a very basic glass, RO, 0.5 SiO_2. The basic mixtures were not like glasses; they were more like stones. I then tried adding alumina to one of these same stony basic slags. The first addition reduced the stony condition greatly; the second produced a beautiful clear glass. In the third addition, the glass had gone beyond the point of maximum fusibility, and particles of white unfused matter were floating around in the clear matrix of the glass. Further additions of alumina made it stony again. There seemed to be a point where alumina made a stony basic slag into a perfect

glass, but also, it seemed equally clear that too much alumina converted the glass back to the stony condition again.

I value Professor Binns' paper very highly, and shall value it more as I have opportunity to study it. I have never done much experimenting along this exact line myself. I had one student who pursued the subject last summer for two months in a somewhat desultory fashion. He started to make a matt glaze, and attacked the problem from several different points of view. He first tried to determine whether mattness came easier from extremely basic or extremely acid glazes, and he came to the conclusion that it was easier to make a matt from the basic end of the series. The general formula of the glaze he finally reached was something not far from where Professor Binns wound up;—about thirty-five hundredths of alumina, and one and a half of silica. While this work was merely the work of a student, the same general ratio is borne out by his results.

The Chair: To what temperature did this student carry his experiments?

Professor Orton: Nought two.

The Chair: And Binns', nought one.

Mr. Thos. Gray: Series Four of Binns' trial-pieces seems to have a strong resemblance to the defective ware we got, and we decided a few days ago that more fire would be beneficial, and yesterday sent them to the kiln to be fired again. They seem to be much like one of these samples of Mr. Binns' We redipped a few, and they seemed to come out all right; so we sent the whole lot down yesterday to be fired again. The kiln was fired so rapidly on account of burning soft coal; our burners were not used to such a fuel and let the heat get away from them.

Mr. Karl Langenbeck: Of course it is possible that with the glaze having a tendency to devitrify if the conditions of firing were not closely maintained, devitrification of the glaze would explain the phenomenon you speak of. On the other hand, I have seen a raw glaze when fired with a very sulphurous coal, take on a slight scum of lead sulphate on the surface. I think a good many phenomena might look like this, and still come from very different causes.

I merely point out these differences, because I think it would be to the great interest of the members to get some kind of a classification of crystalline or amorphous separations from glass. In the first place, we need some nearer definition of glass than that of an amorphous silicate, which is very broad; then some information as to the kinds of separation which take place. There is such a phenomenon as the separation in crystalline form of an oxide which has supersaturated the glaze, as in crystals of chromic oxide in chrome aventurine. Also, there is the separation of sulphates from the glaze in crystalline form. There is the beautiful crystalline separations of which so much artistic use has been made recently by the Royal Copenhagen works, and again the separation of alumina in matt glazes. I should like to ask if anyone has made any observations on these lines, as to a possible classification?

Mr. Stanley G. Burt: I think a still further possible explanation of Mr. Gray's trouble is that the rapid firing may have brought about a reducing effect. If he had a certain per cent of lead in his glaze and had a smoky fire, he might have reduced it, and got a matt surface. He could then put it back in the kiln, and get it all right.

Mr. Gray: But, we have put it in again, without doing anything to it.

Mr. Burt: You re-oxidize it, by refiring it, and thus overcome the reduction of the first burn. I am a little disappointed that Professor Binns did not go into the crystalline structure of the matt glaze, and I agree with Mr. Langenbeck that it is a great field for study. We are undoubtedly getting at it more and more closely. The crystals of the Copenhagen ware are zinc crystals, as I know, and I also know of chromium crystals. The matt glazes which have come under my observation are almost invariably traceable to crystalline effect. I rather get from Mr. Binns' paper, tho it is a little hard to follow such a paper, that he has still in his mind some suggestion of suspension of alumina particles. To my mind, it is not a question of suspension purely, but more of crystallization; and in the study of the different glaze-crystals we arrive at the explanation of the different

grades of mattness. For instance, no one would call the Copenhagen glaze, however beautifully and completely crystallized, a matt glaze. Nor would you call a piece of the chrome-iron glaze, matt; nor would you call the Italian mosaic glass (aventurine) which is now made in New Jersey, I understand, a matt glaze. But, I feel that the matt glazes shown here by Professor Binns are very probably due to the fact that there are a number of bases all crystallizing out together, giving in that way a great mass of broken crystals together.

The Chair: Have any of the members had a chance to examine these samples of Professor Binns' under a high power glass.

Professor Edward Orton, Jr.: Some few of these crystals can be seen by the naked eye; but I think the crystalline formation of matt glazes must be proved by slicing and grinding, and then examining under a microscope, rather than by external appearance.

Mr. Karl Langenbeck: Some very interesting studies have recently been made on the crystallization of alloys of copper and aluminum; and the character of the alloys was brought out in a very interesting manner, by photographing the sections and projecting by means of a lantern on a screen. I think the suggestion is a very meaty one; and if some one having a connection with a minerologist who has a rock sectioning apparatus, is willing to prepare some of these for projection upon a screen, I think some very interesting things would appear.

Professor H. A. Wheeler: I think Mr. Burt's opinion of Mr. Gray's trouble is probably right. He likely had a reducing action. We also may have overlooked the fact that in glazes where there is a high percentage of alkaline earths, and the glaze is nearly saturated with them, letting the fires go up and down may result in crystalizing out. It is less likely to occur than the other explanations, but still that condition of affairs might arise in a kiln being burned by a careless burner, who did not keep his fires uniform.

Mr. Isaac Hardy: I understood Professor Binns to say that lead is indispensable to a matt glaze. The glaze I am

using to make what my firm calls satin-finish brick, consists of feldspar, flint, zinc oxide, calcium carbonate, and is burned up to cone six, and it will stand burning to cone ten and still give a matt finish. We take ten to twelve days to cool the kiln off. On some bodies, this glaze will come bright; on other bodies it will come matt; and again it will come semi-bright.

Professor Wheeler: Is you lime pretty high?

Mr. Hardy: I don't recollect just what it is. It is not very high—there is about forty per cent. of feldspar, I believe. If I had had copies of these papers beforehand I would have come prepared to discuss them. But as this is my first meeting, I did not know what line of proceedure would be followed, or I should have come fully prepared to talk.

Mr. Karl Langenbeck: The statements of the last speaker remind me of some experiments I made a number of years ago, starting with Seger cone four as a basis for hard-fire colored glazes. I recollect I substituted zinc oxide—small amount at that—for the lime, which in general would produce a glaze similar to what Mr. Hardy describes; and I got matt glazes then. And in connection with this matter, I particularly noticed in the glazes stained with oxide of manganese and oxide of copper, that the turbidity and texture of the matt glaze is more conspicuous than in any of the others.

Mr. Isaac Hardy: I do not get my matt finishes by the addition of alumina, but rather by the addition of silica. We have one glaze which comes quite bright, but by the addition of silica it changes to a matt finish.

Professor Orton: I think we all realize that mattness may proceed from a number of different causes. I think confusion has been brought about by considering lead glazes on the one hand, and another type of crystalline glazes on the other. They are not at all alike. In Bristol glazes such as Mr. Hardy and Mr. Langenbeck have just described, the effect may be reached by the addition of silica, while in lead glazes, it is reached by diminishing the silica. The same laws do not hold good for both kinds of mattness.

Mr. Karl Langenbeck: Is not the relation of alumina to silica a question of temperature? Say the firing is from cone nought five to cone nought one: in that case, starting with enough alumina to prevent devitrification, that is, the crystallizing out of the silica under long fire, the matt glaze would be produced by adding an excess of alumina. But in the case of a Bristol glaze, fired from cone six to eight or nine, while you have a large alumina content in comparison with the alumina content in the lead glaze, yet it is in all probability nearer to the devitrification point. If the proportion of alumina to silica is not high enough, a small increase in silica brings about the result mentioned. We must take temperature into consideration.

Mr. Alfred Yates: I will give a little experience I had in Johnsonburg. I had succeeded in getting a very bright glaze and I gave a copy of the receipt to a friend in New York. He put it on his body, and got a dull finish—the most beautiful glaze I ever saw. He used the same glaze, the same quantities of the ingredients, but used on two different bodies, the results were entirely different.

DEATH VALLEY, CALIFORNIA, AND ITS BORAX INDUSTRY.

By Edward Hart, Ph. D., Easton, Pa.

The Chair: Gentlemen, we are assembled here to listen to a lecture, illustrated by lantern views on the screen, by Professor Edward Hart, Professor of Chemistry at Lafayette College, Easton, Pa., and last year elected a member of this society. Many of you are acquainted with Professor Hart, by reputation if not personally, and those who have not had the pleasure of a personal acquaintance with him have missed one of the good people of the profession. We will now listen to Professor Hart. (Applause).

Dr. Hart: Gentlemen of the Ceramic Society.—For several years my friend Mr. Stover has been sending me every year about Christmas time some beautiful product of their factory at Trenton, and every time there has been a meeting of the Society, he has asked me for a paper. This year there was no present, and when Professor Orton asked me for a paper, I took the hint and will give you the best I have.

The country I am going to speak of is a very peculiar country. Before speaking of Death Valley, I shall take the liberty of saying a word about Adamana and the big trees there. While silica is one of the materials of the potter, it does not seem likely that we will ever be making commercial ware out of petrified trees. But as this discourse is only semi-scientific, I will bring it in on this pretense.

In all this southwestern country, the light is very strong due to the absence of moisture from the atmosphere, and, not being accustomed to photographing under such conditions, my pictures are all over exposed. On these deserts the surface soil has little or no covering, and the wind and rain have an excellent chance to play havoc. So it happens that while the rainfall is small, yet coming as it does in heavy downpours, it causes heavy erosion. The whole surface being ex-

PLATE 1

PLATE 2

INDIAN HIEROGLYPHICS NEAR SARATOGA SPRINGS.

(See Page 69.)

posed, anything that can be carried off by water is carried away; the water will carry nearly its own bulk of solid matter. The evidence of erosion is so great, that you would think the rainfall tremendous.

The first cut (see fig. 1) gives a good general idea of the appearance of the country. We visited a place near Adamana, where the ground was littered with pieces of tree trunks, which have in the course of many years been washed out of the mesas or table topped hills which abound. In the mesas, the trunks are found lying horizontally, and nearly unkroken, in a layer about fifteen feet from the surface, in a bed of what seems to be volcanic ash, below a capping of sandstone conglomerate. This sandstone seems to fall apart as the edge becomes exposed, probably under the influence of the frost, and is washed away. As the support below the trunks washes away unevenly, they break apart crosswise, never lengthwise, into sections of a foot to eight feet long. At one place (shown in the second cut) a trunk perhaps four feet in diameter and 120 feet long spans a ravine which has been excavated beneath it.

When we landed at Adamana, we found six inches of snow. I was determined to see the forest, however, even if I had to dig it out, and my determination was justified, for by the time we reached the forest we found the snow nearly melted off. On a rough guess I should think the trunks might cover about a thousand acres, the largest trunk I saw was about eight feet in diameter. There were no small branches—nothing less than three inches in diameter—and some of the trees showed evidence of decay before petrification began. I concluded, therefore that the trees must have fallen and the leaves and small twigs decayed before the silica solution which must have caused petrification began its work.

The driver informed us that there are four other petrified forests in the vicinity, one of them larger and finer than the one we saw. The forest we saw has been examined by experts in such matters who think the trees were Sequoias, but some pieces I saw, said to have been taken from the other forests, looked more like oak.

The accompanying rough sketch map will perhaps serve to give a good general idea of the country traveled over. It is a country of rough stony mountains and sandy valleys. The altitude at Daggett is about 2,000 feet and at Cave Wells about 4,000 feet. Death Valley is a little below sea level.

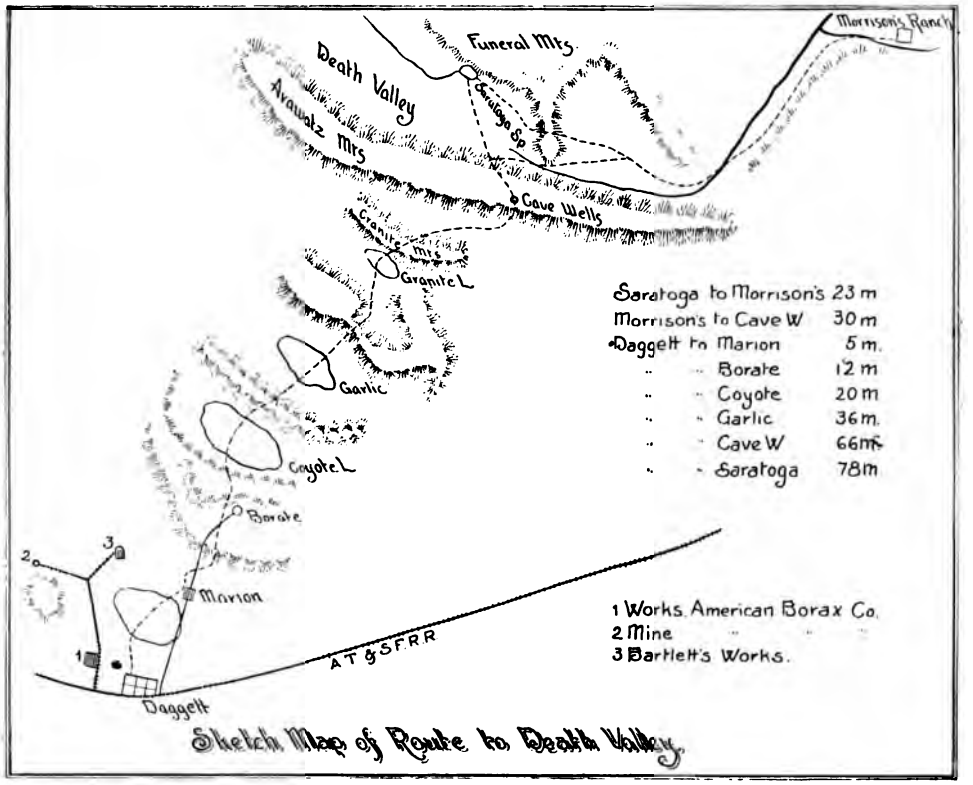
I arrived at Daggett on a Monday morning, at 5 o'clock, and found the town asleep. It is not an attractive looking place—no hotel, many saloons, and a general go-as-you-please look. I had hoped to be able to drop my title here but it was not to be. Eisenberg, one of our boys, had been at the borax works and some of the books we print at Easton were in their small library, so I was professor there too and after the first shock the people took it as well as could have been expected.

From here to Death Valley the trail runs in an almost straight line to the northwest, over several divides and across several playa lakes, gradually rising until it reaches the summit of the Avawatz Mountains. nearly 4,000 feet high, which border Death Valley on the south.

Daggett is situated on an open plain and is said to be on the bank of the Mohave River. To the uneducated eye the so-called river is nothing but a bed of sand. Like many of the streams in this arid land the river burrows in the sand and steals along next to bed rock beneath the surface. Dig there and you find water in abundance.

The trail from Daggett passes over the Mohave, then over a stretch of sand, and then over a playa lake to Marion, 5 miles. A railroad also runs to Marion, owned by the Pacific Borax Co.

These playa or dry lakes are a peculiar feature of this peculiar country. They contain for most of the year no water, the surface being fine, dry mud. The water from the surrounding country carries this fine mud during the infrequent downpours, and this is deposited as the water stands. Part of the water evaporates and the rest sinks below the surface, carrying with it most of the dissolved material. Very little evaporation takes place through this fine compact surface deposit. Most of it occurs at the edges of the lakes where the sand is coarser, and here the alkali crusts are to



Sketch Map of Route to Death Valley.

be found, Some of these playas are covered with cracks, others are almost entirely without them. They are perfectly level and the roads cross them in a straight line. When in good condition, as we found them, they make perfect roads, hard and smooth, and I believe one horse could pull 5 tons on such a road with ease. Coyote Lake was the largest lake we saw and is perhaps 5 miles in the shortest and 12 in the longest diameter. Many thousands of tons of alkali must be contained in the deposits around its edges and still more dissolved in the water beneath its surface.

At Marion the Pacific Borax Co. has built a mill for roasting the Colemanite. Colemanite is a borate of lime, containing water. On roasting, it loses the water and falls to a fine powder which can be sifted out. The valueless mud, mixed with some of the Colemanite, does not fall to a powder when heated and in this way the Colemanite is purified. The fine powder is put in sacks and sent to Bayonne, N. J., where it is treated with sulfuric acid which combines with the lime. The boric acid goes into solution in the hot water, which is poured off, and as this cools the boric acid crystallizes out. The boric acid is then sold as such for enameling iron, for pottery, for glass-making, etc., or it is boiled with carbonate of soda and so converted into borax. The Colemanite treated at Marion comes from Borate, 12 miles to the northwest of Daggett. From Borate to Marion it is carried on a narrow gauge road running along the trail. The grade is very heavy and the road crooked, crossing the trail repeatedly. As the engine comes shrieking around among the mud hills it looks like a veritable demon and frightens horses and driver. This country is full of boric acid, which seems to have accumulated in the ancient lake which once covered all this country, and has since completely dried up. At one place, 7 miles northwest of Daggett, an old lake deposit, 60-90 feet thick, is found standing now almost vertically. It is a hardened mud and was originally deposited as such on the old lake floor. It contains 10 per cent. of boric acid and is worked at Daggett, where it is mixed with water, treated with sulfur dioxid, made by burning sulfur, and the solution allowed to evaporate in shallow tanks. No artificial

heat is needed, the sun does the evaporating at a tremendous rate. This is easily understood. The temperature reaches 118 in the shade and there are frequent winds with a velocity of 40 miles and more an hour.

Most of the borax used in the United States comes from this little town of Daggett, where there are three producers. In 1901 we used a little over 18,000 tons of borax, of which probably nearly 17,000 tons came from Daggett.

My outfit for the trip to Death Valley consisted of a team of two horses, such as the horseman calls chunks, and a driver, known locally as Tug—a lad of 19. I paid $5 a day for the team and $3.50 to the driver. Food and water for ourselves and fodder for the horses we carried with us. We had also a mess kitt and our beds, but no tent. The bed is made as follows: First a strip of canvas 14 feet long and 5 feet wide is spread on the ground. On this is placed a mattress and pillow, on these a woollen blanket folded bag-fashion from the bottom. On this several blankets are placed and the canvas folded up over all. You find a level place, scrape away the stones, undress quickly, for the nights are cool, crawl inside and draw the canvas over the head. In the morning you dress, wash your face, if there is any water, and you are ready for the road. The bed is rolled up, tied with a rope and thrown on the wagon.

The amount of bread and butter eaten on this trip astonished me. Tug was no cook and I knew little more about it. We both loathed washing dishes, so that coffee and bread and butter formed our staple diet. We started out bravely with eggs and bacon, but to eat eggs and hog fat and then to clean that frying pan were too much for us, especially where water was scarce and time short. Our camp-fires were made of sage brush or grease wood. We tried cedar at one camp but did not like it. Sometimes it is hard to find wood enough to burn, and as for trees there are none—I saw nothing as thick as three inches.

We followed the trail used by the borax teams carrying supplies to their ranch in Death Valley, and crossed the playa lake to Marion, where we filled our canteens. Then to Borate and then to Coyote Well, on the border of the playa

PLATE 3.

1.—Fording the Amaragosa, looking north-west.
2.—Valley of the Armaragosa, looking west.
Avalvatz mountains in the distance.
3.—Sand Cliffs on the Amaragosa.

lake of the same name, where we watered our horses. Then on over a divide to Garlic, where we camped. This was thirty-six miles for the first day. Next day we pushed on over two divides to Cave Wells, in the Avawatz Mountains, where we watered the horses, and then to Saratoga Springs, which lies on the northern edge of Death Valley. From Cave Springs to the valley is a heavy grade down hill, 4,000 feet, and then across the alkali flats, perhaps two miles wide at this point. This day's journey was forty-two miles and the horses were very tired. Saratoga Springs bubble up at the very foot of the Funeral Mountains, which enclose the Valley on the north. The water is warm and contains some dissolved material, a little borax among other things. It is not very good for men, but horses can and do drink it. There are a few small fish in it and the puddles around it, called lakes here, are the resort of ducks and mud hens. Ten feet from the springs there is an alkali crust six inches thick. There is a small hay barn at Saratoga Springs where hay is kept for the borax teams. This hay comes from Morrison's Ranch. After supper I spread my bed on the hay bales inside while Tug spread his outside nearer the horses. About 8 o'clock we were roused by the noise of a team which turned out to be Morrison himself with a four-horse load of alfalfa, weighing 3,000 lbs. net, which he had hauled twenty-three miles from the ranch over very rough roads since 11 o'clock the same morning.

Next morning I climbed the mountains to look at the Indian hieroglyphics with which the rock are covered while the others were breaking camp. During the ride I visited, riding sometimes in one wagon and sometimes in the other. I looked upon Morrison with some curiosity. He has spent several summers here and told me that it was pretty hot sometimes—128° in the shade.

Until we reached Saratoga Springs our trail ran northeast. From there for about twelve miles it ran almost due east, crossing a low spur of the Funeral range with wonderful sand dunes lying on one flank in the distance, then crossing the Amaragosa river and curving to the north following the river. Up to the crossing of the Amaragosa, the

valley is very wide, but above that it contracts to a narrow canyon, hemmed in by walls of sand cemented together into a rock-like mass by alkali. A few miles below this crossing the Amaragosa, which has the volume of a small mill stream and contains much dissolved alkali, disappears under the sand.

Morrison's ranch, where we stopped that night, is on Willow Creek, about half a mile above its junction with the Amaragosa. The ranch contains about twelve acres under cultivation—mostly alfalfa—and is surrounded by clay hills in which nitrate of soda is found. The surface of these hills is like loose ashes for a depth of six to twelve inches. Below that a hard crust is found, containing the niter six to twelve inches thick and as hard as iron. In these hills and those lower down the valley, Bailey estimates the total amount of nitrate so far located as equal to 22,000,000 tons. At the present time, all our nitrate for making gunpowder, nitric acid, gun cotton, nitroglycerin and celluloid comes from Chile.

We imported in 1901 233,720 tons nitrate of soda from Chile, so that at the present rate of consumption these California deposits contain enough to supply our wants for nearly 100 years.

Within a short time a new railroad from Salt Lake to Los Angeles will cross this valley and then these salt deposits can find their way to market. On the way to Morrison's we met a party of twenty-five prospectors belonging to the American Nitrate Company, and at the ranch we found as many more encamped. At the head of the party was Mr. Norton, who goodnaturedly offered to loan me his bed, which I declined with many thanks. Next morning bright and early we left for home, going back over our route out for most of the distance. On the way Tug discovered a tarantula, who had been out too late and been overtaken by the cold night air and benumbed, and I took his picture. Tug said that when he was smaller, one of the Daggett doctors bought tarantulas for four bits, or fifty cents each, and the boys caught them as follows: The tarantula burrows in the sand, with a trap-door closing his hole two inches from the sur-

PLATE 4.

1.—The Amaragosa River, sinking into the sand.
2.—Sand Cliff on the Armaragosa.
3.—Morrison's Ranch, Nitrate Hills in background.

face. One boy creates a disturbance around the door and the tarantula rushes up, closes it and holds on, then the other boy inserts a shovel below him and throws him out on the sand, when he is quickly caught in an empty bottle.

The first day of our return journey was Christmas day. In the morning it was cold, but by noon the temperature was at 80°. We had bread and butter and canned salmon for dinner, and for dessert I prospected and found a gypsum deposit, while Tug smoked cigarettes.

That night we slept at Cave Wells. The rocks here are infested with rats, and they held high carnival, running back and forth over our beds. In the summer this must be a lively place. Tarantulas are plentiful, and there is a snake which Tug called a "side winder"—poisonous.

Next day we reached Coyote Wells on our return, forty-six miles—our longest day's journey. On the way we met and passed the borax team taking supplies to the ranch. There were twelve mules and two lead horses. In the old days, twenty mules pulled two wagons hitched tandem, the mules two abreast. These wagons could carry, besides supplies and water, 52,000 pounds. Such a team travels about twenty miles daily. The cost of freighting in the desert is about twenty cents per ton mile. Next morning it was very windy. Breakfast was gotten ready under difficulties, and the wind was very strong for several hours. Then it moderated and we rode into Daggett at high noon, tired, hungry, and dirty. I was very much indebted then to Mr. Henry Blumenberg, superintendent of the American Borax Company, for he added to much previous kindness, the loan of his bath-tub, one of a very few—perhaps the only one in Daggett.

Next morning at 3 o'clock I boarded the train and at the Harvey eating house, at Los Angeles, gorged on dainties and rejoiced that I need not live in Death Valley.

DISCUSSION.

Mr. Karl Langenbeck: What is the nature of the soluble salts, in the sand banks on the Amaragosa?

Dr. Hart: I do not know of my own knowledge. None of my samples have come to hand. Bailey's report, which

treats of the matter pretty fully, states that they vary in composition. Salt is a prominent constituent. There is sulphate of soda there, carbonate of soda in places, tho not much, I think. These seem to be mixed up in varying proportions. On the sides of the wash, leading out of the Valley, are salt deposits of considerable extent. There is plenty of gypsum there, too. But the whole country seems to be liberally scattered over with these materials, and it looked as tho it had been thoroughly stirred afterward.

The nitrate deposits are covered by claims, bought up by the American Nitrate Company, which will soon be operated. A new line is to be run from Salt Lake City to Los Angeles. It will cross this country about midway between the different nitrate deposits, which it will be possible to reach by narrow guage roads from the main line.

Mr. Langenbeck: By nitrate, you mean nitrate of soda?

Dr. Hart: Yes.

Professor Edward Orton, Jr.: Where do they get the water supply for leaching out these beds?

Dr. Hart: There is nothing of that kind done in the Valley now. No practical work, nothing except prospecting, has been done in the Valley except the hauling of Colemanite to Mojave. We found a prospecting party of about fifty people belonging to the American Nitrate Company, and there were several parties from the Pacific Borax Company in the field, but no practical work is being done. The factories are at Marion and at Daggett. The American Borax Company is at Daggett.

Mr. Langenbeck: All the borax at present made comes from these borax deposits?

Dr. Hart: Yes. The Colemanite mines of the Pacific Borax Company are at Borate, about twelve miles from Marion. The Boric acid made at the Bartlett works is made from the borax mud deposits of that region. The American Borax Company's mine lies about seven miles from Daggett, connected by a narrow guage road. The ore is brought to Daggett and there turned into boric acid. The water at Daggett comes from the Mojave river. The river there is

under the sand. They sunk wells, found the river and have plenty of water.

Mr. Stanley Burt: The refining is completed right there? Do we receive it direct from there?

Dr. Hart: Boracic acid is shipped from there mostly to Pittsburg. But the American Borax Company's work is so far only in the experimental stage. Mr. Blumenberg told me he had a three month's run last summer, and found what he could do, and is now going ahead on the basis of that.

Mr. Langenbeck: Mr. Burt's question is of interest to users of boracic acid for ceramic purposes. It has transpired that there is a good deal of sulphate of ammonia in some boracic acid of Italian origin, which has caused disagreeable results.

Dr. Hart: Yes. Ammonia occurs as a sulphate in the Italian deposits. I have no knowledge of such impurity in the California borax; the samples I secured there have not yet come to hand, and so I have not been able to make any analysis yet.

Professor Orton: Is there any animal life there, to speak of?

Dr. Hart: My driver told me that in the summer time it is quite interesting. The tarantulas at Cave Wells are very thick, and it is difficult to find a place not already preempted. The night I was there, the rats or mice had a jamboree, and raced across my bed a number of times. I think probably in the summer time there is a good deal of life. I saw nothing but a few mud hens, a single tarantula, and I think three chipmunk and a lizzard which came out in the middle of the day. But there is evidence there of a swarming animal life in the summer time.

Professor Orton: I move that the Society express its gratitude to Dr. Hart for the exceedingly interesting lecture he has given and for the care and trouble he has taken in preparing it for us by a vote of thanks.

Motion seconded. and unanimously adopted.

RECENT INVESTIGATIONS IN PORTLAND CEMENT.*

BY ALBERT V. BLEININGER, B. SC.

It is a difficult task to collect the experimental data produced by the many investigators who have worked on the examination of the hydraulic silicates and especially of Portland cement. Keen scientific minds, equipped with thorough training, have applied their energy to the solution of the various problems connected with the Portland cement industry. As a result, we have before us a mass of material representing an enormous amount of work and energy, and differing widely in scientific value; the investigation of the thoroughly equipped chemist and mineralogist is found in the various journals, side by side with the speculations of the obscure and often untrained observer. In addition, these multitudinous results are found scattered in many different periodicals in three or four languages. If, in attempting to produce an outline of the work recently done in this direction, the task is not complete, it is due to the great amount of material in existence, and to the fact that it is scattered over such a wide territory.

It is a peculiar fact that the great bulk of the work in the investigation of Portland cement has been done along theoretical lines, as distinguished from similar work done by the metallurgists on steel, where the first problems attacked were strictly practical in character, and have only of late developed a phase which might be said to be strictly scientific, almost entirely freed from empiricism. The investigation of the true character of Portland cement seems to have had a great fascination for the very first observers.

Since Vicat (1857) Candlot, Fuchs, Feichtinger, many experimentors have endeavored to arrive at results, which might throw light on the vexed question of the constitution of cements with but indifferent results, since most of them endeavor to make use of wet reactions. All of them found

*Published in advance, by permission of Edward Orton Jr., State Geologist of Ohio.

that the calcareous cement compounds are extremely unstable. Michaelis, in the sixties, has shown that even alcoholic solutions of salts in the absence of water decompose the hydraulic compounds. Up to about 1887, all that was known concerning the hydraulic cements might be summed up in a few sentences, viz:

Natural or artificially prepared mixtures of silica, alumina and carbonate of calcium, when heated above redness, produce compounds which harden in contact with water. The hydraulic compounds produced are calcium silicates and perhaps aluminates, which unite with water, chemically, giving rise to a mixture of hydrous salts.

It is not until the aid of the microscope was called in (1887), that more definite statements could be made, although all this while, the gross composition necessary for a good Portland cement was known.

In this year Chatelier[1] published his classical work on the constitution of Portland cement, based on microscopic analysis, in which he states that he found the main constituents of Portland cement to consist of, (1) colorless double-refracting, cubical crystals; (2) between these a darker substance, double-refracting, but without crystalline structure. In addition, he found several accessory constituents,

(a) slightly yellowish crystals, opaque and showing striation,

(b) very small crystals with rather strong double-refraction, and

(c) finally zones of matter without influence on polarized light.

Constituent No. 1 was called, later, alite; No. 2, celite; a, belite; and, b, felite. Chatelier considers the alite as $3\ CaO\ SiO_2$, this being the active element of cements, par excellence, which hydrates according to the following reaction:

$$3\ CaO\ SiO_2 + Aq = CaO\ SiO_2\ 2.5\ H_2O + 2\ Ca\ (OH)_2.$$

In addition to this compound, the Portland cement contains a tri-calcic aluminate, which is relatively unsta-

1 Recherches experimentales sur la constitution des mortiers hydrauliques. Annales des Mines 1887.

ble, but sets rapidly in water, and if present in too large amounts, may cause the destruction of the cement. It hydrates according to the reaction:

$$3\ CaO\ Al_2O_3 + Aq = 3\ CaO\ Al_2O_3\ 12H_2O.$$

Chatelier thus considers Portland cement as composed of tri-calcium silicate, with a certain amount of calcium aluminate and ferrate, besides mono-and di-calcium silicates. The hydration of the cement he expresses by the two reactions:

$$2\ (3\ CaO\ SiO_2) + 9\ H_2O = 2\ CaO\ SiO_2\ 5\ H_2O + 4\ Ca\ (OH)_2.$$

Then a reaction takes place between the calcium hydrate, water and calcium aluminate forming a basic calcium aluminate:

$$3\ CaO\ Al_2O_3 + Ca\ (OH)_2 + 11\ H_2O = 4\ CaO\ Al_2O_3\ 12\ H_2O.$$

The hydrated basic aluminate determines the setting of the cement, while the hardening is fixed by the tri-calcium slicate. Chatelier fixes the amount of lime necessary for Portland cement by the expression:—

$$\frac{CaO}{SiO_2 - Al_2O_3 - Fe_2O_3} > 3$$

in which CaO, SiO_2, $Al_2\ O_3$, $Fe_2\ O_3$ represent the number of equivalents of these substances present. The other limit he fixes by the expression:—

$$\frac{CaO}{SiO_2 + Al_2O_3 - Fe_2O_3} < 3$$

Toernebohm checked the petrographic work of Chatelier, but did not agree with the latter as to the composition of the tri-calcic silicate.

W. B. and S. B. Newberry[1] agreed in the main with Chatelier's results, but took exception to his tri-calcic aluminate, which they replaced by bi-calcium aluminate, so that they obtained the formula:—

$$X\ (3\ CaO\ SiO_2) + Y\ (2\ CaO\ R_2O_3.)$$

Dr Tomei[2] treated hardened cement with an aqueous solution of ammonium chloride and found that the amount of lime dissolved decreased after some time. He concluded from his experiments that the stability of the lime compounds increases with the time and that part of the silica

1 Journal of Society for Chemical Industry, 1897.
2 Thonindustrie Zeitung 1895, page 177.

from the sand of the mortar, enters into combination with the lime.

In 1895, A. Hauenschild[1] treated a number of hydraulic cements with various solutions of ammonium salts and found that these salts did not permit of an exact determination of the lime compounds.

The work of Toernebohm and Newberry was condensed by Meyer (1896) in the formula:—

$$X\ (3\ CaO\ SiO_2) + Y\ (5\ CaO\ R_2O_3\ SiO_2)$$

This investigator objected to the assumption of aluminates and ferrates, since alumina and ferric oxide always form a double silicate in the presence of lime.

Microscopic examination in no case showed the presence of free lime.

Prof. Rebuffat,[2] of the University of Naples, in 1898, divides cement, into two classes:

1. Cements of simple, compact structure, non-crystalline, derived from a mixture of calcium oxide, silicate, and aluminate. The calcium oxide may be absent. These comprise the hydraulic limes and quick setting cements.

2. Cements of crystalline structure derived from a combination of a crystalline mass, consisting of $2\ CaO\ SiO_2$ and CaO and the calcium aluminate in various proportions—Portland cements and cements high in silica.

He assumes that the qualitative constitution of the different hydraulic cements after setting is the same. They are formed from a mixture of lime hydrate, hydrated calcium silicate and hydrate calcium aluminates with a slight quantity of accessory ingredients. The calcium silicate is the ortho-silicate—$2\ CaO\ SiO_2$. The quantity of the water of hydration which is absorbed does not exceed 5½ per cent. so that the hydrated compound has the formula $2\ CaO\ SiO_2 + H_2O$. In cements high in silica, the presence of a certain quantity of meta-silicate must be accepted. This does not hydrate, but reacts with the aluminates and forms double silicates of calcium and alumina. This fact is important, as it explains the resistance of cements to sea water.

1 Thonindustrie Zeitung 1895, page 289.
2 Gaz. Chim. Ital. 1898.

The calcium aluminate may have one of the following formula:—

$$CaO\ Al_2O_3.$$
$$2\ CaO\ Al_2O_3.$$
$$3\ CaO\ Al_2O_3.$$

But unless there is an excess of lime or unless certain conditions of burning prevail, it seems that only $CaO\ Al_2O_3$ and $2\ CaO\ Al_2O_3$ should be considered.

The quantity of water chemically fixed by hydration cannot be determined beforehand. The cements hydrate in different degrees, according to whether they harden in pure or lime water. To this are due many peculiarities in setting.

In setting, the calcium ortho-silicate is hydrated to form $2\ CaO\ SiO_2 + H_2O$, which is followed by the hydration of the calcium aluminate. These two reactions take place in different phases for different cements.

With cements of crystalline structure, it must be assumed that the calcium ortho-silicate is found combined with CaO and the calcium aluminate in a definite crystalline condition. Portland cement in the hydrated condition contains but small quantities of calcium aluminate. This explains that in cements of crystalline character the setting may be caused by the hydration of the calcium ortho-silicate according to the following reaction:

$$2\ (3\ CaO\ SiO_2) + 3\ H_2O = 2\ (2\ CaO\ SiO_2)\ H_2O + 2\ Ca\ (OH)_2$$

This reaction is associated with the hydration of the aluminate, which in Portland cement consists mainly of mono-calcium aluminate and hydrates with seven molecules of water.

$$CaO\ Al_2\ O_3 + 7\ H_2\ O = Al_2\ O_3\ CaO\ 7\ H_2\ O$$

Rebuffat found that all the water of crystallization does not become free at low heat, but part is expelled at a bright red heat.

Prof. Rebuffat's work rests on the assumption that the free calcium oxide can be removed from a cement by means of a sugar solution of certain strength, an assumption which has not been verified by the work of Michaelis and Feret[1] who throw strong doubts on this method.

1 Internat. Congress for testing building material, Paris, 1900.

Michaelis in 1892 and Erdmenger in 1893, have shown that the bi-calcium silicate in a cement gives rise to "dusting," that is, it breaks down to a powder without showing hydraulic properties. This is supported by the later work of Newberry. Hence, cements low in lime or such whose silica content is enriched by the silica from the ash of the coal will show this disastrous behavior. But from further experiments, Erdmenger came to the conclusion that there must be an allotropic modification of the bi-silicate, since the same cement low in lime when cooled suddenly by quenching in cold water did not dust. The same change from one state of the bi-silicate to the other is determined by the length and intensity of burning. As soon, however, as more lime is drawn into reaction and the cement approaches the empirical tri-calcium silicate composition, the phenomenon of dusting disappears.

In 1898 Prof. Carl Zulkowsky,[1] of the University of Prague, also published his results on the investigation of cements. He believed that he could remove the free lime from cements indirectly, by using dilute hydrochloric acid. His main argument, however, is based on the study of blast furnace slags. His conclusions are summarized as follows:

I. The blast furnace slags, suitable for the manufacture of cement are highly basic meta-silicates whose decomposition is prevented by rapid cooling—granulation.

II. Owing to their anhydrous condition they have a tendency to unite with water and to harden, which is assisted by the presence of alkalies. Such silicates may be called hydraulites.

III. On slow cooling an inter-molecular decomposition of the meta-silicate or hydraulite takes place in a greater or smaller measure. During this process the mass loses its uniformity and the constituents are not dissolved by weak acetic acid in the proportion in which they are present in the slag.

IV. The hydration of the powdered basic meta-silicate is co-incident with a change in volume and a change in the

1 Die Chemische Industrie 1898, page 69.

shape of the grains which now fill all the space and gradually harden to a compact mass.

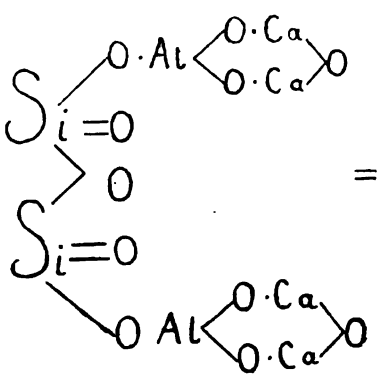
V. Slags cooled slowly, which contain only certain components or isomeric compounds of the above meta-silicates, do not possess the properties of the hydraulite, mentioned under II and IV.

VI. Portland cement, like slag cement, is a mixture of a hydraulite analogous to granulated furnace slag and with as much lime as remained uncombined on burning. The difference between the two is only that the necessary lime must be added to the slag in the shape of calcium hydrate.

VII. The CaO of Portland cement cannot be separated directly.

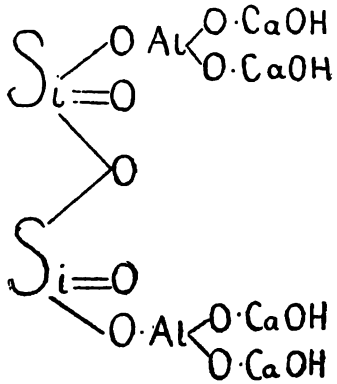
VIII. The calcium oxide may, however, be removed from Portland cement by a special treatment with dilute hydrochloric acid in greater or smaller amounts, and the hydraulic property is diminished accordingly. The residue remaining behaves like highly basic slag; that is, it hardens in the presence of the alkaline compounds without combining with them chemically.

IX. But since the calcium oxide is thus removed in the wet way, the cement is sufficiently hydrated and the residual cement can never attain its original strength. The hydraulic agent of Zulkowsky is represented by the structural formula:—



$= 4Ca\ 2SiO_2\ Al_2O_3$

This compound hydrates to:—



This formula is thus seen to be deduced from a basic meta-silicate which hydrates by the taking up of a molecule of water.

In 1899 Dr. Michaelis[1] published a startling theory which contradicted all the previous assumptions. He claimed that the hydraulic hardening is a combination of chemical and physical processes. For the main constituent, the hydro-silicate of lime crystallization cannot be proven notwithstanding the claims of Chatelier and others. The process of hardening depends upon an increase in volume on the part of some of its constituents, or the formation of gelatinous silicic acid. On letting a large quantity of water act on Portland cement, the volume of the latter is changed to a colloid mass occupying 33 times the original volume. On treating cement with lime water, the volume is increased 21 times. Even quartz, when treated with milk of lime, is converted into the colloid variety in which it is greatly increased in volume. The same is true of alumina and iron. Michaelis cites the manufacture of sand brick, by the action of calcium hydrate on crystalline silica as a proof of his theory. His claims, however, have found no approval by most of the cement chemists and investigators.

In 1897 Liamin,[2] a Russian investigator, made an extensive investigation of Portland cement by means of micro-

1Thonindustrie Zeitung 1899, page 785.
2Report of the Imperial Russian Technical Society 1897.

scopic examination, physical separation and chemical analysis. He made photographs of micro sections of hardened Portland cement, in which he showed the crystals of calcium hydrate. By means of a suspension method employing heavy liquids like methyl iodide and benzol, he washed out the calcium hydrate from the finely ground cement. This work he attempted to check by the determination of the combined water in the hardened cements, assuming that the hydrous lime silicates lose their combined water at about 160° C, while the calcium hydroxide loses its chemical water at from 450-480° C. In this way, he calculated the amount of calcium hydrate present by a very ingenious method of reasoning. These results apparently checked beautifully with the values obtained by the physical separation. In five Portland cements, he found up to 33.6 per cent. of calcium hydrate. The hardening, he claims, is due to the interlacing of crystals of lime hydrate and a lime alumina silicate. The important factor is the calcium hydrate, which he found in crystals about one millimeter long. Roman cements do not show this phenomenon to any extent. The content of calcium hydrate increases slowly since the higher calcium silicates gradually change to simpler compounds and lime hydrate. Thus the $3\ CaO\ SiO_2$ of Chatelier is reduced to the simple $CaO\ SiO_2$ occurring as fine, hair-like crystals, and hexagonal crystals of $Ca\ (O\ H)_2$.

Many investigators object to this extraordinary high content of calcium hydroxide and it has been shown by work done by the Ohio Geological Survey that the dehydration of cement is not as sharp and smooth a process as accepted by Liamin and that hence his dehydration values are erroneous. The last traces of combined water are expelled only at a red heat.

In 1899 a great number of experiments were made to determine the free lime in Portland cement for all investigators realize that if once it is possible to determine the free calcium oxide, the entire question of cement constitution is solved.

B. Steuer[1] allowed hydrogen sulphide to act upon water in which finely powdered Portland cement had been poured. In this manner, he extracted 44 per cent. of calcium oxide which he considered free lime while the residue corresponded to the formula $CaO\ SiO_2$.

S. Wormser and O. Spanjer[2] used as reagent for the solution of free lime, an alcoholic solution of aluminum chloride. They claimed to have proven that this solution (1 g in 100 cc's. absolute alcohol) does not decompose the silicates, but dissolves only free lime. The per cent. of CaO found to go in solution as such was 25, while in addition 20 per cent. of lime was found present as a highly basic silicate. They calculated by means of their method, a content of 26.6 per cent. of free lime, and 18.65 per cent. of CaO, combined with 6.15 per cent. silica.

Dr. Hart[3] in the same year, by means of a 10 per cent. alcoholic iodine solution found, he claims, 30.34 per cent. of free vitrified calcium oxide in Portland cement; the balance of the cement material he considers as ballast, a slag which does not harden; on hydrating, this vitrified lime hardens under water. He cites this free lime as a fine example of a body whose chemical energy is manifested, not by a thermal reaction, but by a mechanical development of energy. The reason why so much free calcium oxide is not injurious to the cement, he explains by saying that this lime is vitrified and crystalline. Vitrified, crystalline calcium oxide and the amorphous lime, though not distinguished analytically, are different physically. This argument is open to many objections, whose discussion here would lead too far.

Dr. Hart's work was supported, however, by thermochemical experiments, in which he determined the heats of neutralization of Wollastonite $CaO\ SiO_2$, artificial $CaO\ SiO_2$, $2\ CaO\ SiO_2$, $3\ CaO\ SiO_2$, $CaO\text{-}Al_2O_3$, $2\ CaO\ Al_2O_3$, $3\ CaO\ Al_2O_3$. He determined the heats evolved for the anhydrous cement as well as for the hydrated and dehyd-

1. Thonindustrie Zeitung 1899, page 604.
2. Ibid, page 1785.
3. Ibid, pages 659, 770, 858, 1569; 1900, page 188.

rated cement which had been allowed to harden. The heat of neutralization of the hardened, not dehydrated cement should be higher than that of the clinker, since the former contains a considerable amount of free calcium hydrate. In Portland cement the heat of neutralization is lowered on hydration, while the heat evolved by the dehydrated cement is almost equal to that of the clinker. From this Dr. Hart concludes that from 30 to 34 per cent. of free lime must be present in cement.

Dr. Michaelis objected to the use of wet reagents for the determination of free lime, claiming that they cause grave inaccuracies.

Dr. Wegner[1] allowed carbon di-oxide to act upon cement at a red heat, in a furnace fired by gasoline. He attempted to determine the free lime by this method and found 11 per cent. of CaO. This work obviously is of but little value since a constant temperature of 800° C is required, which cannot be said to have existed in his furnace, and to be of value, the experiments should have been conducted in an electric furnace.

Wormser[2] studied the reaction of ammonium oxalate on calcium aluminates and silicates, as well as on Portland cement at a red heat. He claims to have found the composition of Portland cement to be:

14 per cent. $CaO\ Si\ O_2$
43 per cent. $2\ CaO\ Si\ O_2$
23 per cent. $2\ CaO\ Al_2\ O_3$
20 per cent. CaO.

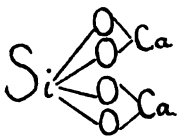
In 1901 A. Meyer[3] published his interesting account on the formation of cement, in which he summarizes his theoretical and practical experience. First a mono- and bi-silicate of calcium is formed, followed by the formation of an aluminate and ferrate of lime; then a tri-calcium silicate is formed accompanied by the crystallization of the stable double-silicate of lime and alumina. After the formation of the tri-calcic silicate, only bi-silicates can be produced. Be-

1. Transactions Society German Portland Cement Manufacturers 1901.
2. Ibid.
3. Bull d. l. Socite des Sciences de Boucarest 1901, No. 6.

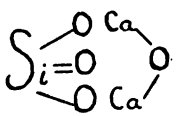
side this, an isotropic lime-alumina-silicate may be formed. The time of burning and cooling is of great importance on the reaction. The more basic the mixture, the less fusible will it be; hence, the higher the required temperature. A normal cement may dust if heated too long, while if burnt rapidly, it will show the proper behavior. If the amount of bi-silicate is too great, the cement will dust; this formation of bi-silicate can be prevented by rapid cooling. In many well burnt Portland cements, the main mass is a devitrified glass, in which several minerals in well developed crystals are observed.

A basic furnace slag, granulated, does not represent a devitrified glass, but a true bi-calcium silicate and aluminium silicate.

Everything tends to show that the bi-calcium silicate may exist in two forms:

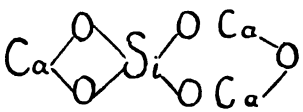
Ortho Silicate 

and

Meta Silicate 

The latter hydrates, since it contains the lime as anhydride. If exposed to a higher temperature, it changes to the crystallized ortho-silicate, which falls to a powder. In cooling blast furnace slag rapidly, we retain the meta-silicate, thus preventing the change to the ortho-silicate.

In Portland cement, which is not granulated, the formation of the non-hydraulic bi-calcium silicate is prevented by adding so much lime that the tri-calcium silicate is produced:



In this, two-thirds of the lime is hydrated. But, so far, nobody has succeeded in producing the tri-calcium silicate by direct synthesis. Newberry could not have had it, owing to the imperfect mixture of the ingredients used, and to the lack of all analytical data regarding it; Rebuffat produced a mixture of less basic silicates and free CaO. Chatelier produced it indirectly with the use of fusing calcium chloride.

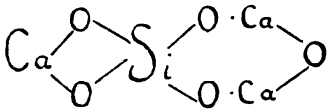
Edward Jex[1] in 1900 claims that the ortho-calcium silicate must be the main hydraulic agent of Portland cements. A meta-silicate is always produced by fusing a cement mixture, and an ortho-silicate can arise only in vitrification when greater resistance is offered to the motion of the molecules, so that each molecule is surrounded by several molecules of calcium oxide. If the clinker is heated for a longer time, the ortho-silicate reverts to the meta-silicate and hence the quality of the clinker is lowered. This difficulty is overcome by introducing more lime than is necessary for the ortho-silicate. This is a purely mechanical but good means of preventing the meta-silicate formation. But it is doubtful whether it would not be better to be rid of this excessive amount of lime. According to this investigator, the resulting compound in cement would be:

$$2\ CaO\ SiO_2 + 2\ CaO\ Al_2O_3$$

The dusting of clinker he explains by the presence of a mixture of meta- and ortho-silicate, which possess a different constitution, tending to break up the mass. Jex also suggests the possibility of producing a Portland cement by fusion, in kilns similar to a blast furnace. The raw material could be introduced in the shape of crushed rock, while the cement mixture is discharged in the liquid condition and granulated. The fuel consumed would be about 1 .5 times the amount used in the rotary kiln, but the process would do away with the power consumed in the fine grinding of the raw mixture.

1 Thonindustrie Zeitung 1900, Nos. 138, 136, 139.

In 1901 Th. Ludwig[1] compares cements with glasses, which might be called alloys. It is his opinion that the formula of A. Meyer:



represents the proper chemical conception of one of the glass constituents, the alite, which may crystallize out, though he denies that any experimentor has ever produced this silicate in the pure condition. He also supports Meyer's formula:

$$X\ (3\ CaO\ SiO_2) + Y\ (5\ CaO,\ R_2\ O_3\ SiO_2.)$$

replacing the modified Chatelier formula:

$$X\ (3\ CaO,\ SiO_2) + Y\ (2\ CaO\ Al_2\ O_3).$$

He doubts the presence of ferric silicate, claiming that the iron is always present as ferrous oxide.

This has been disproved by Loebell, and by work done by the writer for the Ohio Geological Survey, in which the iron was found in several cements to be largely in the ferric condition.

In the same year, Dr. Rohland examined Portland cement from the stand-point of hydrolysis, but arrived at no definite conclusions.

In 1901 Zulkowsky[2] again published a most valuable series of experiments whose results may be summarized as follows:

The existence of the tri-calcium silicate is not probable and not proven. The di-calcium meta-silicate is the true carrier of hydraulicity, which means the presence of free calcium oxide. (He could not produce the tri-calcium silicate, but did not use fluxes). Silica with a valence of six does not exist. The tri-calcium silicate is not necessary for the production of a good Portland cement; in fact, Zulkowsky produced a high grade white Portland cement of the formula 2 CaO SiO_2, 2 CaO Al_2 O_3 from kaolin. Any lime above this is unneces-

1 Thonindustrie Zeitung 1901, No. 142.
2 Zeitschrift fur Chemische Industrie p. 290.

sary. He compares Portland cement with an alloy, in which CaO is in solution. Hence there might be a distinction between alloyed and unalloyed lime, which will slake in different ways. In presence of an excess of calcium oxide, the cement contains dead-burnt and alloyed lime. It is nearly certain that the first will slake like the hydraulites, the second much later, like dead-burnt lime, giving rise to "blowing."

In 1902 Rebuffat[1] gives an elaborate account of his latest studies, in which he pays special attention to the rapid-setting cements of the type of the natural cements. He started to produce these cements synthetically by the use of kaolin and calcium carbonate, heating them to about 800° C. In this connection, he found the remarkable fact that crystalline calcium carbonate, or even lime hydrate, when ground together with kaolin and burnt, did not give rise to hydraulic cements at all. After experimenting for a considerable time, he found that neither of the above forms of lime reacted sufficiently, and subsequently he was compelled to use amorphous calcium carbonate, precipitated by ammonium carbonate. The crystalline calcium carbonate reacts only at higher temperatures. By using amorphous calcium carbonate, he obtained synthetic cements, with kaolin, which showed satisfactory behavior.

This work of Rebuffat agrees with the work of the writer done for the Ohio Geological Survey in 1902, up to a temperature of 1100° C, at which point, crystalline calcium carbonate was found to react with mixtures of kaolin and flint, producing hydraulic cements equal in strength to the natural cements. There is no doubt, however, that amorphous calcium carbonate would increase the strength attained considerably. Rebuffat found that free silica is not necessary in the production of hydraulic rapid-setting cements, as mixtures of kaolin with 3, 4, 5, 6 and 7 molecules of calcium-oxide give very quick setting cements—the most rapid one being, one molecule of kaolin to six molecules of CaO, burnt at 800° C.

1 Thonindustrie Zeitung No. 105.

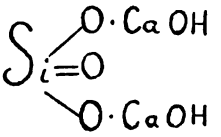
Rebuffat also treated all of his cement mixtures with sugar solution, by means of which he expected to extract the lime and he determined the chemically combined water of the various lime compounds. Rebuffat's synthetic work may be summed up as follows:

On heating mixtures of kaolin and calcium carbonate from 800 to 1200° C, the change in the time of setting is equivalent to a change in chemical reaction, as indicated by the increasing amounts of chemically combined water. The pozzulanic character of the natural cements is due to the presence of clay substance, and not to the free silica.

Rebuffat extended his experiments to natural rock cements, and arrived at the following conclusions:

Natural cements contain amorphous calcium carbonate. A synthetical mixture of SiO_2, Al_2O_3, and amorphous calcium carbonate, does not produce a rapidly setting cement. A mixture of previously prepared silicates and aluminates is not hydraulic at 800°, hence the natural cements are not mixtures of silicates and aluminates. All facts tend to show that natural cements are lime-alumina silicates, which above 800° gradually change to a mixture of calcium silicate and calcium aluminate; a change completed at 1200° C. If the mixture contains so much lime that all of the alumina becomes bi-calcium aluminate, the product at 1200° may retain a certain rapidity of setting.

A. Meyer[1] in 1902 reports the production of tri-calcic silicate from a fused magma, which hydrated to $2\,CaO\ SiO_2\ H_2O$; at 160° C, half of the fixed water is expelled, which indicates that the molecule must be a double one. The remaining water leaves at a much higher temperature than 200°. Hence it is impossible to have a meta-silicate—



for here the two hydroxides are united with lime and behave analogous to calcium hydrate, which loses its water at a much higher temperature.

1 Thonindustrie Zeitung No. 144.

The hydration equation of the main constituents of Portland cement can be said to be:—

1. $2\,(3\,CaO,\,SiO_2) + Aq = 2\,(2\,CaO\,SiO_2\,H_2O) + 2\,Ca\,(OH)_2$

2. By washing out the calcium hydrate the reaction can be carried to the following limit:—

$2\,(2\,CaO\,SiO_2\,H_2O) + Aq = CaO\,2\,SiO_2\,3\,H_2O + 3\,Ca\,(OH)_2$

The presence of a tri-calcic silicate is more readily to be assumed than that of meta-bi-calcium silicate and excess of lime. The alumina and lime are firmly united with SiO_2 and hence there are present no aluminates or ferrates.

In 1902 N. Liamin[1] investigated the phenomena connected with the change in the rate of setting of cements and studied the effects of salts like $CaCl_2$, $Ca\,(NO_3)_2$, $(NH_4)_2\,CO_3$, etc. He arrived at the conclusion that salts producing insoluble compounds with $Ca\,(OH)_2$ accelerate setting, while those forming soluble salts retard it. The same subject was studied by Dr. Rohland[2] last year. He ascribes the change in the rate of setting:

1. To the disintegration of the coarser cement particles to finer ones.

2. Change in the condition of the aluminates.

3. Excessive amount of alumina.

4. Formation or introduction of one or more catalytic agents.

He makes a list of catalytic agents, positive or negative, accelerating or retarding the hydration, including—

Calcium sulphate	—
Aluminium chloride	+
Sodium carbonate	+
Calcium chloride	+
Barium chloride	+
Potassium bi-chromate	—
Calcium chromate	—
Sodium chloride	0

The relation between the already existing velocity of reaction and the effect of a plus or minus catalytic agent is expressed by the statement:

1Thonindustrie Ztg., 1902, No. 66.
2Thonindustrie Ztg., 1902, No. 49.

Where the velocity of reaction (setting) is very slow, the catalytic agent produces a greater effect than in those in which the velocity is greater. Positive and negative reagents may neutralize each other. The quantities may be very small, one per cent. or less is often sufficient to produce great changes.

Dr. Bernhard Kosmann in 1902 discusses the constitution of Portland cement, and says that—"the imaginary tri-calcium silicate must be a basic meta-silicate in which two molecules of CaO are present, so that they are split off by the combination of water."

The study of the effects of sea water on Portland cement has occupied the attention of many European authorities owing to the great importance of the subject. The opinions in this respect are very much divided; some defend the Portland, claiming that good cement amply resists the dissolving action of the sea water, like Schuljatschenko, of Petersburg, while others like Chatelier, Rebuffat and Michaelis, seek to determine the weak points of the cement or to find a suitable remedy.

Thus Michaelis advocates the use of puzzolanic material, like trass, to be ground together with the cement. Chatelier claims that the starting point of the solution of the cement is the formation of calcium aluminium sulphate. This assumes dangerous proportions only if the content of alumina rises to four per cent., hence the alumina should be replaced by iron oxide. The dangerous quality of the alumina is decreased by an increase in lime and completely eliminated by silicious puzzolanes.

Rebuffat ascribes the action of sea water to sulfo-aluminates and says that the process is but a slight and superficial one, which does not harm the cement work to any appreciable extent.

In the United States, activity along the line of cement research was quite pronounced in the past year.

W. B. Newberry[1] examined the progress of burning in the rotary kiln by cooling a kiln sixty foot long, without emptying it. A constant reaction was observed from 840°C,

1 Cement and Engineering News 1902.

being shown by the change in color, and a constantly decreasing amount of volatile matter. Fourteen samples were taken at equal intervals. The most lively evolution of CO_2 took place between intervals 8–12(1030°–1425° C). The raw cement mixture at three was of a blue gray color, which between three and four becomes buff, retaining this color up to eight, when it changes to brown; at nine the lumps become harder; at eleven, they are vitrified on the outside, at twelve partially vitrified; at thirteen black.

Dr. Clifford Richardson (1902) has prepared synthetically non-dusting di-calcium silicates, as well as 3 CaO SiO_2 both hardening in moist air and water.

Prof. E. D. Campbell, of the University of Michigan, has carried on for several years, elaborate tests of cement mixtures, burning them in an experimental rotary kiln accompanied by accurate temperature measurement.[1]

"The minimum temperature required to burn Portland cement giving a sound cement with fresh clinker is 1450° C, for minimum contents of CaO. The temperature increases with the lime till in ordinary cements, it reaches 1550° C. The substitution of Al_2O_3 and Fe_2O_3 for SiO_2 lowers the overburning temperature, but may raise the temperature required for perfect boiling test. With a mixture low in CaO, the burning temperature for perfect boiling test is lowered by this substitution, but with mixtures high in calcium oxide the burning temperature required for the perfect test is raised and may become coincident with the overburning temperature. Any attempt to raise the overburning temperature by increasing the lime will fail to give a perfect hot test, even at the overburning temperature. The practical experience that lean clays (higher in silica) are safer than rich clays is verified. The effect of 8 – 9 per cent. MgO has but little effect on the temperature for perfect boiling test or over-burning temperature."

Professor S. B. Newberry assisted by Mr. M. M. Smith, also published a most valuable series of experiments.[2]

1 Journal American Chem. Society No. 10, 1902.
2 Cement and Engineering News, Nov. 1902.

He again reports having prepared 3 CaO SiO_2 by mixing the finely ground materials dry, and heating the mixture to a white heat. It does not, however, harden readily. No direct chemical proof of the 3 CaO SiO_2 is given. By fusing the mixture over the oxy-hydrogen blow pipe, the 3 CaO SiO_2 was found to be double-refracting, with rectangular cleavage, spec. gravity 3.022, is constant in volume, hardens well. A mixture of calcium carbonate and silica, heated for two hours at a red heat, leaves some SiO_2 uncombined unless at least 2½ molecules of CaO to one of SiO_2 are present. At a white heat, the mono-, di- and tri-silicate mixtures become completely combined. The amount of water which a given cement will take up chemically, is a function of the mechanical conditions, porosity, etc. It may be as low as 10, or as high as 27 per cent.

On suspending finely divided calcium silicate in sufficient water to dissolve all the lime present, the residues are somewhat indefinite in composition, and continue to lose lime on prolonged action; they have, however, the following composition:

Tri-silicate (not fused), after twenty-nine and ninety-three days, 3 CaO 2 SiO_2 3 H_2O.

Tri-silicate (fused), 35 and 228 days, 2 CaO SiO_2 H_2O.

The di-silicate (quenched and dusted), did not change in composition, and did not hydrate.

The action of an amount of water insufficient to dissolve all of the lime present (100 cc's to 5 g) on the fused tri-silicate, for thirty days, followed by a treatment with sugar solution for 29 and 186 days, gives a residue approximately, 3 CaO 2 SiO_2 3 H_2O and 3 CaO 2 SiO_2 6 H_2O.

The work done for the Ohio Geological Survey in 1902 by the writer, though not completed, may be indicated by the following outline. It will form the substance of a special bulletin of the survey in the near future.

In the presence of clay substance, evolution of carbon di-oxide from amorphous calcium carbonate commences slightly at about 650° C, a drop in the rate of evolution is reached at 800° but resumed at 850°. At 1000° C the evolu-

tion of carbon di-oxide is completed. The lime begins to react with clay substance soon after the evolution of carbonic acid begins, the clay being completely decomposed at about 850° C. Free quartz begins to be attacked at about 950° C. Free silica coarser than the size between 120-150 mesh sieve is inert, at least at temperatures up to 1350° C. Feldspathic minerals are decomposed almost as readily as clay substance. Crystalline calcium carbonate is much more sluggish in reaction than the amorphous variety, especially at temperatures below 1100° C.

The chemical activity of calcium oxide was studied by grinding calcium carbonate intimately together with flint in a wet ball mill, so that a mixture of the formula 0.25 CaO SiO_2 was produced. This was burnt at 800°, 900°, 1000°, 1100°, 1200° and 1350° C, measured by means of the Chatelier pyrometer, these temperatures having been maintained for ninety minutes. After burning, the mixture was analyzed for each temperature, the total silica, the silica soluble in hydrochloric acid and sodium carbonate, the alumina and iron, and the carbon di-oxide being determined. The ground flint was examined in regard to its fineness of grain and was found to possess the following composition:

Left on 120 mesh sieve - - -	1.40%
Left on 150 mesh sieve - - -	2.36%
Left on 200 mesh sieve - - -	1.06%
Grains .0088 to .0004 inch diam. - -	24.74%
Grains .0016 to .0004 inch diam. - -	17.84%
Grains .001 to .0001 inch diam. - - -	9.30%
Dust below these sizes - - - -	43.30%

From the amount of soluble silicic acid and the amount of calcium oxide, the formula of the compound resulting from the action of the lime on the silica was calculated. It was assumed that owing to the large excess of silica, and to the intimate blending, the calcium oxide would show its maximum chemical effect under the above conditions of fineness of grain. These formula of course do not represent the act-

ual formula produced, since the exact amount of lime entering into reaction must remain an unknown quantity.

TABLE NO. I.

Temp. °C.	Per Cent. Soluble Silica	Per Cent. Available CaO	Sum of Soluble Silica Plus Available CaO	Carbon Di-Oxide Per Cent.	Formula
800	5.94	8.94	14.88	6.60	$CaO\ 0.7\ SiO_2$
900	6.44	16.40	22.84	1.30	$CaO\ 0.36\ SiO_2$
1000	3.62(?)	18.04	21.66	.....	$CaO\ 0.19\ SiO_2$
1100	17.11	18.04	35.15	.....	$CaO\ 0.887\ SiO_2$
1200	22.10	18.04	40.14	.....	$CaO\ 1.14\ SiO_2$
1350	22.73	18.04	40.77	.....	$CaO\ 1.18\ SiO_2$

This table seems pretty consistent with the exception of the 1000° determination, which is now being repeated. At 800°, apparently all of the free calcium-oxide has combined with silica, while at 900° there is evidently some lime uncombined, so that the formula appears distorted. It is evident the lime has united with no less silica than at 800°, but more calcium oxide is available than at the latter temperature. This is indicated also by the caustic property of this mixture. The best index of the activity of the lime is the amout of silica rendered soluble, especially if for 800° and 900° the carbon di-oxide is eliminated by calculation which will make the soluble silica 6.36 and 6.52 per cent. respectively.

Intimately associated with the progress of the chemical reaction between the calcium oxide and the silica, is the evolution of heat connected with it, at least for temperatures below 1300°C, when the lime is rendered inert as far as the measurable heat reaction is concerned. The heat produced must be ascribed mainly to the hydration of the calcium-oxide since it has been found by the writer that lime silicates evolve but little heat as will be shown by the following table of heats of hydration, determined on commercial cements. The values are expressed in calories per gram of cement, an excess of water having been used, 100 c.c. for from 3 to 5 grams. The cements, except when stated otherwise, were purchased in the open market and represent standard American brands, with the exception of one.

TABLE II.

HEATS OF HYDRATION.

No.	Kind of Cement	Calories Per Gram	Remarks
1	Portland Cement	1.786	
2	" "	1.190	European Cement.
3	" "	0.470	
4	" "	2.000	
5	" "	....	
6	" "	0.500	Obtained from manufacturer.
7	" "	2.100	
8	" "	2.200	
9	" "	5.560	Did not stand up well in boiling test.
10	" "	5.160	Did not stand boiling test.
11	Natural Cement	4.100	
12	" "	3.770	
	Natural (Magnesian) cement rock burned in large laboratory furnace for 1½ hours at		
13	400° C	No heat.	
14	500°	0.950	
15	600°	1.430	
16	700°	0.830	
17	800°	0.800	
18	1000°	0.470	
19	1100°	1.580	

The use of the calorimeter has been found of great value in judging a cement in regard to its fitness for use. I invariably use it for examining cements, before testing them. Owing to the cheapness of a good serviceable instrument and the comparative rapidity of a determination, this method of testing should find application by manufacturers.

Heat determinations were also made of various calcium and magnesium silicates which are given in the following tables:

TABLE III.

Composition	Temperature	Calories Per Gram	Composition	Temperature	Calories Per Gram
0.25 $CaO\ SiO_2$	700	1.60	2 $CaO\ SiO_2$	800	50.00
"	800	1.98	"	900	30.37
"	900	1.60	"	1000	2.50
0.5 $CaO\ SiO_2$	1100	0.88	"	1100	1.20
"	1170	0.50	"	1170	0.79
1.0 $CaO\ SiO_2$	600	.....	2.5 $CaO\ SiO_2$	600	2.96
"	700	2.98	"	700	7.54
"	800	1.70	"	900	29.37
"	900	7.34	"	1000	73.46
"	1000	19.85	"	1100	14.20
"	1170	0.80	"	1170	6.80
1.5 $CaO\ SiO_2$	600	0.70	2.8 $CaO\ SiO_2$	700	1.40
"	700	10.71	"	1100	72.85
"	800	15.08	"	1170	53.67
"	900	67.88	3 $CaO\ SiO_2$	700	0.91
"	1000	57.56	"	1100	27.96
"	1100	2.76	3.1 $CaO\ SiO_2$	1000	33.70
"	1170	1.19	"	1200	3.63
2. $CaO\ SiO_2$	600	.....			
"	700	1.59			

These mixtures were prepared from flint whose mechanical composition has been given and whiting containing 1.66% MgO, ground together for 8 hours in a wet ball mill. The heat evolved by the whiting alone was found to be 220.0 calories at 900°C, 195.5 at 1200. This decrease is due to the fact that some silica was absorbed by the whiting from the crucible.

Mixtures of whiting and 20 per cent. of kaolin were made, in order to study the effect of the clay substance upon the decomposition of the calcium carbonate, and the following hydration values were obtained:

TABLE IV.

Temp. ° C	Calories per gram.	Temp. ° C	Calories per gram.
700°C	.....	900°	126.43
800°	32.13	1000°	128.52

Similarly to the calcium compounds, magnesium silicates were prepared and tested in the same manner. These mix-

tures consisted of commercial magnesium carbonate and flint, finely ground together.

TABLE V.

Composition	Temperature	Calories Per Gram	Composition	Temperature	Calories Per Gram
0.5 $MgO\ SiO_2$	500°C	16.33	1 $MgO\ SiO_2$	800°C	5.55
do	600	10.10	do	900	.....
do	700	2.64	do	1000	1.58
do	800	1.78	do	1100	1.78
do	900	1.68	1.5 $MgO\ SiO_2$	500	20.15
do	1000	0.99	do	600	24.51
do	1100	0.90	do	700	12.30
1 $MgO\ SiO_2$	500	18.20	do	800	4.36
do	600	28.19	2 $MgO\ SiO_2$	500	17.27
do	700	5.71	do	600	.....

The magnesium silicates slake with extreme slowness and hence claim to satisfactory exactness cannot be made for the calorimetric determinations. The calorimetric hydration values of both the lime and magnesium series cannot be used as the basis of exact calculations of chemical composition; they can indicate only certain general tendencies, and hence are useful to this extent. In order to afford exact proofs, the determinations must be carried on much more extensively, and most laborious precautions must be taken in order to reduce errors to the minimum. In general, it may be said that the more basic the mixtures are, the greater is the error of the determinations arranged in the above tables.

The claim is being made by some that American Portland cements are not made as carefully as European cements, being insufficiently mixed and ground before burning. Though this hardly needs contradiction, the Ohio survey has examined a number of standard American cements in regard to the insoluble residue left after treatment with hydrochloric acid, sodium carbonate solution, and ignition, with the following results:

1. 0.14 per cent.
2. 0.47 per cent.
3. 0.46 per cent.
4. 0.51 per cent.
5. 0.59 per cent.

The most important factor in the manufacture of Portland cement is fine grinding of the raw mixture previous to

burning. In order to determine the actual fineness of raw cement mixtures, a number of samples were collected by visiting plants in operation, and making physical analysis by the Whitney suspension method. The following table will show the results of this work:

TABLE VI.

No.	Materials Used.	Grinding Machines Used	Residue on 80 mesh sieve. Per cent..	Residue on 120 mesh Per cent.	Residue on 200 mesh Per cent.	Grains .0008 to .0004 in. diam. Per cent	Grains .0016 to .0002 in. diam. Per cent	Grains .001 to .0014 in. diam. Per cent	Finer than last size Per cent	Remarks.
1	Ferriferous limestone and shale	Emery mills for fine grinding	2.00	9.89	4.85	16.93	8.96	8.70	48.68	Ground dry.
2	Cement rock and limestone	Griffin mill	16.88	11.57	4.75	16.21	13.29	7.61	30.17	Ground dry.
3	Cement rock and limestone	Ball and tube mill	8.08	7.42	3.68	23.77	17.52	10.26	34.33	Ground dry.
4	Limestone and clay	Ball and tube mill (wet)	7.40	9.56	2.48	17.72	8.96	8.83	45.05	
5	Marl and clay	Wet tube mill	3.04	5.50	5.21	20.81	12.63	9.61	44.72	
6	Marl and clay	Wet tube mill	30.46	4.28	2.17	6.73	10.61	9.31	47.06	
7	Marl and clay	Wet tube mill	2.48	5.23	2.47	16.14	14.22	12.01	47.46	Shells in marl quite evident
8	Marl and clay	Wet tube mill	26.74	6.99	2.13	10.52	9.77	7,67	36.18	

The last column affords an index for judging the degree of fineness of the cement mixture.

The Whitney method of physical analysis can easily be modified so as to become useful in every laboratory. It may be carried out as follows: Take 5 grams of dried raw cement mixture, make up into a thin paste with water and wash through the 80, 120 and 200 mesh sieve respectively. Dry and weigh the residue on each sieve. The portion washed through the 200 mesh sieve is transferred to a 250 c.c. beaker and enough water added if necessary to make 200c.c. of slurry. Stir for one minute, let settle for one minute and syphon off carefully the supernatent liquid. Repeat this operation until the liquid is clear. Remove, dry and weigh the residue. The washings which have been removed are now boiled down to 200 c.c., stirred as before, allowed to settle for 3 minutes and syphoned off. This is continued till finally after 3 minutes the supernatent liquid is clear. The

residue is dried and weighed as before. The third washings are collected, stirred, allowed to settle for nine minutes, syphoned and washed as before. The sediment is dried and weighed. There are now found by sedimentation: First, a sediment, coming in size between the 200 mesh sieve and one minute's settling, which was found to range between 0.0088 inch and 0.0004 inch for quartz; second, a deposit from three minutes settling, ranging between 0.0016 and 0.0002 inch; third, a sediment, nine minutes settling, corresponding to a size of from 0.0011 to 0.00014 inch. The same method has been applied to burnt and ground Portland cement, though in this case, alcohol redistilled over caustic lime was used. The following table shows the size of grain of a number of commercial Portland cements:

TABLE VII.

No.	Grinding machine used.	Residue on 80 mesh sieve. Per cent.	Residue on 120 mesh sieve. Per cent.	Residue on 200 mesh sieve. Per cent.	Grains 0.008 to 0.002 in. diam. Per cent.	Grains 0.002 to 0.0002 in diam. Per cent.	Grains 0.0007 to 0.0008 in. diam. Per cent.	Grains below last size. Per cent.	Removed by screens Per cent.	Tensile strength 1 cement; 3 sand lbs. per square inch in 28 days.
1	Tube mill.....	7.07	14.56	4.45	22.68	19.29	7.36	24.69	25.98	.....
2	Tube mill.....	9.01	15.35	5.09	21.50	20.53	7.06	21.52	29.45	318
3	Tube mill.....	12.12	15.05	7.61	21.11	16.85	5.91	21.37	34.78	303
4	Tube mill.....	14.11	14.57	7.82	22.43	18.95	7.81	19.32	36.49	.....
5	Tube mill.....	3.84	17.64	5.10	25.27	17.56	10.16	20.42	26.58	297
6	Griffin mill.....	3.06	15.41	8.24	28.56	16.48	12.74	15.51	26.72	350
7	Griffin mill.....	9.43	16.91	6.37	25.52	12.18	9.20	18.49	32.71	313
8	Griffin mill.....	5.00	15.42	14.52	27.30	19.10	9.22	14.88	29.53	285
9	Griffin mill..	4.40	11.35	5.18	28.79	21.30	10 01	24.01	20.89	346
10	Griffin mill.....	4.18	13.30	5.07	22.63	14.64	12.31	28.79	22.55	314
11	German cement..	3.88	15.84	7.63	27.63	17.83	13 30	13.89	27.36	281
	Natural cements.									1 cement: 1 sand
12	Mill-stones.....	7.36	9.93	3.53	16.42	13.98	14.74	34.04	20.82	317
13	Tube mill.....	10.53	12.85	8.50	11.39	22.15	20.72	13.85	31.89	299
14	Mill-stones.....	12.50	11.59	4.29	16.52	21.95	17.42	15.76	28.37	265

It is evident that these data are not sufficient to disparage any particular machine, as the mode of operation,

the rate of feeding, etc., govern to a large extent the fineness of the output. The figures given do show however, how fine commercial cements are being ground. The tensile strength of the various cements, of course, is not solely a function of the fineness, but is dependent much more on the proper chemical composition, thorough blending of the raw mixture, good burning and the length of storing.

Besides these tests, the writer has about completed a large synthetic series of cement mixtures, burnt at different temperatures, intended to show the function of the chemical composition in regard to producing hydraulicity.

The first series is to show the hydraulic strength of synthentical cements, of the Roman cement type, burnt at 900°, 1000° and 1100°C. As the basis of the cement composition, there were used the formulae: $X(2.5\ CaO\ SiO_2) + Y\ (CaO\ Al_2O_3)$; $X\ (2.0\ CaO\ SiO_2) + Y\ (CaO\ Al_2O_3)$; $X\ (1.5\ CaO\ SiO_2) + Y\ (CaO\ Al_2O_3)$; $X\ (CaO\ SiO_2) + (CaO\ Al_2O_3)$.

The clayey material was introduced two-fold; first, by kaolin, 46.4 per cent. SiO_2, 39.7 per cent. Al_2O_3, 13.9 per cent. H_2O; second, by a mixture of kaolin and flint, containing 70 per cent. SiO_2, 22.21 per cent. Al_2O_3, 7.7 per cent. H_2O. The silica-alumina ratio in the first case is 1.17 : 1, while in the second, the ratio is 1 : 1.69. The lime was brought in as whiting of the best quality, which of course, is the same as ground limestone.

The mixtures were ground wet in a porcelain ball mill for six hours and heated for two hours at the tempertures given. The results of this series of tests are shown in Table VIII on the succeeding page.

The American natural cements and the presence of so much dolomite in Ohio, suggested the replacement of the CaO molecule by the double molecule $MgCO_3\ CaCO_3$ in equivalent proportion. The experiments were carried out as those of table VIII. A natural Ohio dolomitic limestone was used. The results of this series are given in Table IX.

TABLE VIII.

No.	Clay Base.	Formula of Cement.	Temperature.	Tensile strength 1 cement: 1 sand. 28 days. lbs. per square inch.	Remarks.
1	Kaolin	$\{2.5\ CaO\ SiO_2$ $CaO\ Al_2O_3\}$	900C	.....	Briquets broke Softened.
2	do	do	1000	.....	
3	do	do	1100	.....	Not tested.
4	56% kaolin 44% flint	do	900	181	
5	do	do	1000	225	
6	do	do	1100	.....	Not tested.
7	Kaolin	$\{2.0\ CaO\ SiO_2$ $CaO\ Al_2O_3\}$	900	20	Softened.
8	do	do	1000	183	
9	do	do	1100	.....	Not tested.
10	56% kaolin 44% flint	do	900	182	
11	do	do	1000	218	
12	do	do	1100	111	Set too rapidly
13	Kaolin	$\{1.5\ CaO\ SiO_2$ $CaO\ Al_2O_3\}$	900	37	
14	do	do	1000	108	
15	do	do	1100	46	Softened.
16	56% kaolin 44% flint	do	900	216	
17	do	do	1000	211	
18	do	do	1100	208	
19	Kaolin	$\{1\ CaO\ SiO_2$ $CaO\ Al_2O_3\}$	900	.....	Softened and broke.
20	do	do	1000	96	
21	do	do	1100	71	
22	56% kaolin 44% flint	do	900	188	
23	do	do	1000	166	
24	do	do	1100	46	

A great many interesting features are brought out by the last two tables, which I lack space to discuss. In general, table VIII indicates that the temperatures 900, 1000, 1100°C are too low to bring out the full hydraulicity of the mixtures. The kaolin seems less efficient than the kaolin-flint mixture.

Table IX shows that we are dealing with a narrow temperature range of hydraulic activity. Whether the breaking down of the silicates is due to the two rates of hydration, the slow one of magnesia and the rapid one of lime cannot be said definitely, but seems probable. Here also the kaolin-flint mixture is more effective than the pure kaolin.

TABLE IX.

No.	Clay Base.	Formula of Cement.	Temperature.	Tensile strength 1 cement: 1 sand; 28 days. lbs per square inch.	Remarks.
25	Kaolin	$2.5(CaMg)OSiO_2$ $(CaMg)OAl_2O_3$	850°C	261	
26	do	do	950	320	
27	do	do	1050	148	
28	56% kaolin 44% flint	do	850	175	
29	do	do	950	235	
30	do	do	1050	284	
31	Kaolin	$2.0(CaMg)OSiO_2$ $(CaMg)OAl_2O_3$	850	82	
32	do	do	950	80	
33	do	do	1050	.....	Soft, broke.
34	56% kaolin 44% flint	do	850	251	Works up well.
35	do	do	950	256	
36	do	do	1050	48	
37	Kaolin	$1.5(CaMg)OSiO_2$ $(CaMg)OAl_2O_3$	850	.....	Broke.
38	do	do	950	.....	Briquets broke.
39	do	do	1050	.....	
40	56% kaolin 44% flint	do	850	89	
41	do	do	950	324	
42	do	do	1050	.....	Briquets broken
43	Kaolin	$1(CaMg)OSiO_2$ $(CaMg)OAl_2O_3$	850	.....	Briquets br'ke in clip.
44	do	do	950	.....	do
45	do	do	1050	.....	
46	56% kaolin 44% flint	do	850	337	
47	do	do	950	313	
48	do	do	1050	.....	Briquets broke in handling.

The survey has extended this series to a higher temperature and has also embraced the Portland cements in a similar set of experiments.

The Portland cement series seems to show that high silica clays invariably produce the best cements, provided of course, that the mixture contain the proper lime ratio, is ground sufficiently and burnt thoroughly. The addition of free silica as sandstone is in many cases of great benefit. In order to do this economically, the stone should first be calcined in a vertical or any other suitable kiln and quenched with water while hot. It should be ground separately, being

added to the mixture in a ground condition. This is due to the fact that quartz does not grind well when enveloped by particles of clay.

The addition of a small amount of iron ore often assists vitrification and improves the behavior of poor cements.

It appears that rotary kiln clinker contains the greater part of its iron as ferric-oxide in spite of the black color of the clinker.

Our knowledge of cements must be built up step by step, synthetically and analytically, by a natural reasoning process and cannot be increased by purely hypothetical assumptions. Too much theorizing and not sufficient experimental work has been done.

NOTE BY SECRETARY: This paper was read by title at the Boston meeting and consequently received no discussion. Discussion is now specially invited, and if submitted in writing will appear in future volumes of the transactions.

NOTE ON VAPOR GLAZING.

By Lawrence E. Barringer, E. M. in Cer.,
Schenectady, N. Y.

In a paper on "The Relation Between the Constitution of a Clay and its Ability to take a Good Salt Glaze," which appeared in Vol. IV of the Transactions of this Society, the writer showed the exact relation between the glaze and body of the Logan ware, as well as other facts connected with the process of salt-glazing, and the points brought out may be applied to salt-glazed wares in general. It is the object of the present note to criticise certain statements made by Mr. Wilton P. Rix* in a paper entitled "Note on the Vaporous Method of Glazing Pottery" which to the writer appear far from accurate in the light of his own work along this line.

Mr. Rix ascribes the decline of vaporous glazing to the demand for cheapness and for uniformity of texture and color. This is probably true with reference to salt-glazing fine wares and the process is no doubt in decadence in this country in the stoneware industry. But for coarse wares such as bricks, sewer-pipes, conduits, etc., these very points of cheapness and uniformity put the salt-glaze in a class of its own. For these products there is certainly no cheaper method and the practice is well established and flourishing for this class of clay goods.

After mentioning three distinct processes of vapor glazing, namely, (a) the development of a carbonaceous atmosphere in the kiln, to have a reducing effect, as in lustres, (b) the development of a plumbiferous atmosphere as in smeared wares and (c) the development of alkaline atmosphere as in salt-glazes; and remarking that only the two latter involve the essential principle of vapor glazing, Mr. Rix goes on to discuss salt-glazing and says concerning the temperature employed, "Neither does the action of glazing take place

*Trans. No. Staff. Cer. Soc., 1, 1901-02, 26-38

except at a temperature of about 1200°C." This sounds final and is certainly to be taken as meaning that good success in the work is not attendant upon temperatures lower or higher than that mentioned. In the salt-glazing kilns of Logan, however, the temperature is regularly 1350°C or more,—never less. And in certain sewer pipe works which have come to the writer's notice, the glazing heat is about 1140°C. In the first place a fire-clay is used for the body, and in the second place a shale. Certainly it seems that Mr. Rix should either modify his statement or else specify the nature of the body to which this is applicable.

As to the chemical composition of the body, Mr. Rix recommends *highly* silicious clays for, he says, "The chemical action of the salt upon the ware only takes place upon the silicious particles of the ware." To this I wish to make vigorous objection. An analysis of the Logan salt glaze gives the following composition:

$$\left.\begin{array}{l} 1.326\ Na_2O \\ .003\ K_2O \\ .297\ CaO \\ .003\ MgO \end{array}\right\} \left.\begin{array}{l} 1.000\ Al_2O_3 \\ .078\ Fe_2O_3 \end{array}\right\} 4.418\ SiO_2$$

Where do the other ingredients apart from soda and silica, particularly alumina, come from, if not from the body? It was also shown by the experiments carried out by the writer that silica alone could not be salt-glazed, and that a clay base was as necessary to the operation as free silica. In fact, a combination of clay base and free silica is necessary in the body to be salt-glazed, and the proportions of these must be such that the ratio of alumina to silica will fall within the limits of

1.00 alumina :4.6 silica
1.00 " :12.5 "

As a rule all the constituents of the body are attacked by the salt fumes and there results a soda-alumina-silicate with small proportions of alkaline earths and iron included in the glass.

Alluding to the sand particles in the body, Mr. Rix asserts that the best results are obtained when these particles are very fine, that is, able to pass through a 120 to 140 mesh sieve. Referring again to my experiments along this line I

would call attention to the conclusions arrived after trials of sands of different finenesses (from 40 mesh to floated flint which would not settle in water in twenty minutes) namely, "As regards brightness, smoothness of finish of the salt glaze, it makes but little difference whether the free silica in the clay is fine or coarse," and I have great confidence in the accuracy of the work performed which led to this conclusion. Fineness of sands affects color, but not texture.

Concerning the kiln atmosphere Mr. Rix says, "If the salt is introduced into a reducing atmosphere, the color of the ware must inevitably be dark and blackish in appearance, nor can this result be remedied afterwards." The writer has taken a brick on which was a salt glaze, light green in color, and again placed it in a kiln and the second time it came forth a dark brown. The same brick put into a kiln the third time came out a light green again. Furthermore, the brick in the bag walls of salt-glazing kilns are light in color after some firings, and dark after others. The color of the salt glaze is subject to change, therefore, but whether this can be controlled throughout an entire kiln has not yet been demonstrated.

With reference to cooling the kiln during salting this statement appears in Mr. Rix's paper, "In dealing with salt glazing, the process of the introduction of the salt necessarily involves the loss of heat through the inflow of cold air through the aperatures by which the salt is introduced, and it is possible by this means so far to lower the temperature of the kiln that the surface of the ware is no longer in a condition to favor a chemical action, the salt being merely deposited on the surface. For this reason, it is generally desirable to add the total amount of the salt in two or three portions, increasing the temperature by a light firing between each addition." As to the dissociation of the salt being an endothermic reaction, by which the kiln is very considerably cooled, Mr. Rix says nothing whatever. Why should opening the fire doors to introduce salt cool the kiln more than would be the case in the introduction of coal at each firing? In salting, the temperature of the kiln is unquestionably lowered, but I think there can be no doubt

but that most of the cooling is due to the heat-consuming reaction which takes place during salting and that the kiln can not be said to be seriously cooled by the mere opening of aperatures to introduce salt. In some plants, it is the custom to mix with the salt resin and crude oil, or other substances high in calorific value, to prevent chilling of the kiln during salting by the rapid combustion of these substances. Mr. Ira A. Williams* has shown how the amount of resin and oil required to theoretically supply the heat units consumed by the dissociation of each unit of salt may be calculated by a study of the reactions involved. It would seem that Mr. Rix has overlooked the main cause of cooling during salting.

While the points brought out in my previous paper were based on careful experimental work, covering several months and are reliable statements, I am sure, I do not hold that they are all indisputable by any means. But differing conclusions should come from careful tests and observations and in Mr. Rix's paper no reference is made to any preliminary work upon which he based his statements. In reading the paper, one can only accept most of the assertions as being the personal opinions of the author.

The subject of salt-glazing offers a wide field for experiment, and one that presents much that is indeed interesting.

DISCUSSION.

Mr. Alfred Yates: Do I understand from this paper, that salt glazing is wholly unreliable as to color, and is liable to be dark or bright, or whatever it happens to be? The color is entirely due to the burner. If the burner will properly prepare his fires, rake up and clear them, so he has a perfectly oxidizing flame, and put the salt on it in that state, he will get a bright color every time. That is my experience.

Mr. Karl Langenbeck: Mr. Wilton Rix says that if what you give as the necessary preliminary steps are not taken, then the color cannot be brightened afterwards by an oxidizing fire. And it seems to be the claim of Mr. Barringer that taking a brick that is greenish and not bright, he has put it

*Brick, Vol. XIV, No. V.

into an oxidizing fire and changed the color, which Mr. Rix asserted flatly would not be the case.

Professor Edward Orton, Jr.: Mr. Barringer did not say anything about reliability of color. He said that Mr. Rix had made a statement, which his own experience did not bear out, and that he had found that a salt glaze *could* be reduced and oxidized back and forth.

Mr. Langenbeck: Perhaps Mr. Rix had this practical point in mind; if a fire has been crowded, and the kiln is possibly spoiled so far as the appearance of the ware is concerned, the color of the kiln as a whole cannot be recovered in good condition. Mr. Barringer's statement would be no answer to that fact. Probably Mr. Yates has had some experience in recovering a kiln of ware which had been spoiled by the burner, and has that in mind in making his statement?

Mr. Yates: I know if you have a carbonaceous flame, you will have a spoiled kiln. The fires must be perfectly clear, and if they are, you will get a reliable uniform color.

Professor H. A. Wheeler: Yes, if you salt once or twice, and then rake up and work your fires and get them into proper condition, the second salting will be all right; but don't salt unless the fire is all right.

Professor Orton: Doubtless these recommendations sound very satisfactory, but I do not think they wholly settle the case. I know of one plant where they use natural gas, and maintain probably as nearly perfect oxidizing conditions as can be obtained in any kiln, and yet their results do not follow out the course indicated by Mr. Yates. In this same plant, I have repeatedly seen bricks with green ferrous colorations, mixed with yellow or brown ferric colorations, and yet the firing was said to be done under oxidizing conditions. I mention this as showing that there are a good many unsolved problems in connection with the matter.

I have from that factory a piece of salt glazed ware made in the shape of a funnel, the coloration of which is brown on one side, and green on the other. What is more, the brown covers one half exactly, and the green covers the other half exactly, the line between being as sharply defined as you

could draw with a piece of chalk. I happened to get possession of the piece by asking the manager of the plant how he accounted for these occasional green bricks mixed among the brown ones. He said he did not know. I said, "You always find them in the same relative places, don't you? That is, you find the green ones where they were out of the current of the gases during the cooling of the kiln, and the brown ones where they were well exposed." He said, "Oh no; you are mistaken. You are liable to find the green bricks right in the middle of a wall of brown ones, where the same gases must have bathed the surface of each alike." I expressed incredulity, and said I would like to see an instance, but he said he had no kiln open at that time, but he was perfectly satisfied that he was right.

Up to this point, you can see I was very much inclined to view these green and brown colorations as being purely oxydation and reduction phenomena, as Mr. Yates and Prof. Wheeler seem to think. But, as I still remained unconvinced, the manager brought out this funnel, one half of one color and one half the other. I immediately said that the piece had been pressed in two halves, and that the clay or treatment had not been the same before burning. This he denied, stating that the two halves were made by the same man from clay from the same pile which had been tempered and brought up to him in one quantity and put in a box to keep it from drying out too fast.

If this were all true, I was compelled to admit that bricks might show an equal divergence in color under the same treatment in the cooling or salt glazing. But it seems to me impossible to account for this change of color on the re-oxidation theory.

It seems to me necessary to assume some difference in the treatment of the two halves of that funnel. I don't believe it could be a matter of oxidation and reduction, when it was sitting on the top of the other wares, exposed to the same gases, both in setting and cooling. I cannot see how the gases could strike it in such a way as to make so sharp a line of divison between the two colors. It must be a difference either in the clay body, or in the treatment of the

two halves. A difference in the clay body might easily have occurred, but the manager was emphatic in saying that no such thing did occur. He said one half was pressed just before noon, and the other half immediately after the man came back from dinner.

It might be said that one half was lying longer in the mold, and took up more saline matter from the mold than the other. But Mr. Barringer in his work on this subject made some experiments on this point by adding small additions of calcium sulphate and magnesium sulphate to the same clay. The effect was to make the trial pieces take on an unusually rich brown salt glaze, even where a very small quantity had been added. It was a very much darker, handsomer glaze than when the clay was glazed without the addition of these soluble sulphates. It does not appear from the test that either half of this particular funnel bore any relation to the presence of these salts from the mold. In fact, the color phenomena in this plant are most remarkable, and I think I have said enough to show that it cannot be explained by any superficial recommendations like merely salting when the fires are clear.

Mr. Yates: I want to ask a question about this place. They burned natural gas, you say. Do they burn with a blue flame or not?

Professor Orton: No, sir, the flame in the firebox is white or incandescent. It might be blue, at lower temperatures.

Mr. Yates: If it gets too blue a flame, it becomes almost incandescent. If the kiln is worked by a damper, that creates currents. The minute the burner dampers his kiln, he loses combustion, and instead of a blue flame, he gets a carbonaceous flame, and where that flame strikes, it produces a difference in color.

Mr. Langenbeck: I would like to bear out Mr. Yates' observations, and still claim that something further must be brought to bear, than that the mere method of passing in the gaseous fuel creates uniformity in color. The mixture of gases in a kiln is much like a mixture of liquids in a vessel. Anyone who has played with a mixture of colored

liquids in water, has noticed how long a time the eddies of colored liquid will pass through the water in all directions before making a uniformly colored liquid.

Anyone who have observed with any degree of interest, the red porcelains originally made by the Chinese and Japanese, with a slight addition of copper to their glaze, which under certain conditions is a pale tint, and again a brilliant one, will have noticed this. Their white porcelain body has been glazed by dipping into a uniform glaze in the slush tub, so the glaze must be the same throughout the entire body, but you will find almost invariably that the brilliant red on the reduced oxide of copper is on certain prominent places, looking as though a flame had struck the body and skirted off the depressions. I don't know whether I make myself understood. (Going to a blackboard) We have, for instance, a little vase or tea jar with a section like this, (illustrating) of Seger porcelain. That will be flamed red all around here, (indicating) and up to this lip, and there, and around this turned-up edge of the foot. I could imagine it caused by the swirls of oxidizing flame like (illustrating), and swirls of reducing flame right beside them, causing a softening all around it. We have to keep before us the mixtures of gases in the kiln, which even if it is a gaseous one is still full of swirls and inequalities.

Mr. Stanley G. Burt: Mr. Chairman, two points have occurred to me in connection with this question of a red color being developed on one side of a piece and not on the other. The finest ox-blood red I ever saw, was in a piece gotten by Mrs. Bellamy Storer. She made a piece of ware one morning and put it in the kiln and turned on the gas and then went away and forgot about it. It was well after lunch time when she recollected it, and rushed down to turn off the gas, expecting to find the piece ruined. When she took the piece out of the little kiln, she found the side of the sagger had cracked, and through this crack the gas had played on the ware, striking the piece on one cheek, as Mr. Langenbeck has explained. There is this gorgeous spot of red where this crack was, all the rest of the piece being in the natural green of the higher cupric oxide.

Another point in the paper which to me was very interesting has not been called up, and I would like to hear it discussed. Mr. Barringer makes the statement that the question of fineness of grain of the silica hasn't anything to do with the formation of the salt glaze, but it does affect the color. I think we have not heard from those discussing this paper about this question, and I would like to have it explained. It is a very interesting point.

Professor Orton: In Mr. Barringer's tests, fine silica produced a remarkable lightening in color. Take the clay Mr. Barringer experimented on, and add to it, say, two parts of coarse silica and the color of the ware is little changed. But add the same quantity of fine floated silica, and the color of the salt-glazed piece is very much lighter. The change in color may come in two ways. The same silica being fine, is distributed in many hundred times more grains and thus affects the color of the body, rather than the glaze. And secondly, the sodium may take up fine silica so much more easily than it does coarse silica, that it preys less on the ferrugineous body materials, and makes a lighter, clearer colored glaze. Perhaps both reasons enter into the explanation.

Mr. Isaac Hardy: What little experience I have had along that line bears out what is in the paper. The finer the clay, the lighter the goods; the coarser the clay, the darker the goods. I have never thought much about the matter, but I should think that with the finer clay you would have the denser body; with a coarser clay, a more open body, and the sodium vapors can get more iron out of the clay, and this makes a darker colored glaze.

Mr. Frank W. Walker: Before the discussion on this paper closes, I would like to say that this is the kind of paper that this Society should encourage. The criticism of previous papers, the drawing out of points of difference in the views of different members are of the greatest importance. In that way we will get the real value of the papers which have been presented at previous meetings, much better than from the discussion which takes place at the time the paper is read. I believe this is the first thing we have

had in the way of criticism of a previous paper, and I hope that the example will be imitated hereafter.

Professor Wheeler: The question as to the influence of the fineness of the silica seems capable of very simple explanation. In the case of sewer pipe, if your clay is coarse, you get a very thin film of glass covering and hiding the oxide of iron. The free oxide of iron gives color to the material, and if the flame happens to be oxidizing it will be dark; if reducing, light. If, however, the silica is in a very fine state of division, you will produce such a large amount of glass, and of such high fluxing value, that all the iron will pass into some chemical condition which is not a silicate of iron; in that event, its coloring influence is very slight. You will recall that there are some minerals of very high percentage of iron, but not of strong color, because the iron is in a combined condition. Other minerals, however, with a low percentage of iron, in free state, will have more powerful colors. I think in the one case, the iron is fixed; in the other, partially. I rather think that is the probable solution.

UNDERGLAZE CERAMIC COLORS.

By Edward C. Stover, Trenton, N. J.

To attempt anything like an exhaustive set of experiments in systematic order on Ceramic Colors generally, and the various ways in which they are applied, and to show how each would be affected by the various fluxes and hardening substances used with them, and how affected by glazes, of all the different compositions, and of the varying quantities of each of the constituents, and at the various heats in general use, and also on the different kinds of bodies, would involve trials running way up into the thousands, to say nothing of the stained slips, and stained glazes, as well as other glaze or enamel colors.

Seger has correctly stated that the Ceramic Colors can only be determined by trial. This statement can readily be verified by making some trials to prove any theories that may be set forth regarding colors.

My first scheme of the trials to be made for this paper was based on the use of seven oxides, viz.: Cobalt, Copper, Nickel, Manganese, Chromium, Uranium and Iron, and also a prepared Chrome-Tin pink, making eight different color trials to run. I also expected to use two reducing mixtures; first, a flux taken from J. E. Corfeld's recipe book; and second, a spinel of each color according to the Seger formula. All of these combinations were to be tried on three different bodies, viz.: bone-china; vitreous, hotel china; and white granite. Further, each color was to be tried in four ways—1st print underglaze, 2nd print on the glaze, 3d ground-laid on glaze, and ground-laid under glaze. These were all to be fired in a glost kiln, under seven types of glazes, and were to be fired at six different heats, Cone 02, 3, 6, 8, 10 and 12. But as this line of experiments would involve over 8000 trials, and would necessitate my having the greater portion of them fired by some of my friends, to

get the desired heats, it was absolutely essential that the series of trials should be materially reduced. Finally, the trials as presented here, numbering 1152, were made along the following lines:

The seven oxides above referred to, including the tin-pink, were treated with the Corfeld flux, and as spinels, under nine different glazes, fired at cone 6, and nine glazes, fired at cone 8, on two kinds of bodies, bone-china and vitreous tile, in two ways each, underglaze and on glaze, all fired in glost kilns.

Each trial has been marked in underglaze black on the back, by the cone number, serial number of glaze, by the letter indicating the oxide, and with a dash under the oxide letter indicating underglaze; those without this dash were put on, on the glaze, and all fired in glost kiln. The trials marked with a small $\underline{s}$ were spinel, while those without this letter were half oxide and half Corfeld flux.

The spinel mixtures were made, and calcined in biscuit oven, covered, but to allow air. They were compounded according to the Seger method of spinel as follows:

NiO	0.5	MgO	0.5	Al_2O_3	1.0
CuO	0.5	"	0.5	"	1.0
MnO_2	0.5	"	0.5	"	1.0
Cr_2O_3	0.5	"	1.0	"	0.5
Co_3O_4	0.5	"	1.0	"	0.5
Fe_2O_3	0.5	"	1.0	"	0.5
U_3O_4	0.5	"	1.0	"	0.5
2 CaSn CrO_3	0.5	"	1.0	"	0.5

The Corfeld flux has the following chemical composition:

$$\left.\begin{array}{l} K_2O\ 0.72 \\ PbO\ 0.07 \\ CaO\ 0.20 \end{array}\right\} SiO_2,\ 3.33$$

made as follows:

Flint,	500
Pearl Ash,	100
White Lead,	50
Whiting,	50
Nitre,	50

calcined in glost oven, covered, but to allow air.

Both the Corfeld flux, and the spinel, were very finely ground on glass slabs before being applied.

The nine Cone 6 glazes were all mixed raw from the following formulae:

No. 1—	K_2O 0.3 CaO 0.6 ZnO 0.1	Al_2O_3 0.22,	SiO_2 2.00
No. 2—	PbO 0.3 CaO 0.6 ZnO 0.1	Al_2O_3 0.22,	SiO_2 2.00
No. 3—	Na_2O 0.3 CaO 0.6 ZnO 0.1	Al_2O_4 0.22,	SiO_2 2.00
No. 4—	BaO 0.3 CaO 0.6 ZnO 0.1	Al_2O_3 0.22,	SiO_2 2.00
No. 5—	ZnO 0.3 CaO 0.6 SnO 0.1	Al_2O_3 0.22,	SiO_2 1.00 Sb_2O_3 1.00
No. 6—	Na_2O 0.3 CaO 0.6 ZnO 0.1	Al_2O_3 0.22,	SiO_2 1.40 B_2O_3 0.60
No. 7—	MgO 0.3 CaO 0.6 ZnO 0.1	Al_2O_3 0.22,	SiO_2 2.00
No. 8—	K_2O 0.3 CaO 0.7	Al_2O_3 0.50,	SiO_2 4.00 B_2O_3 1.00
No. 9—	K_2O 0.19 CaO 0.48 ZnO 0.13 PbO 0.20	Al_2O_3 0. 26,	SiO_2 2.68 B_2O_3 0.30

The nine Cone 8 glazes were mixed raw according to the following formulae:

No. 1—	K_2O 0.3 CaO 0.6 ZnO 0.1	Al_2O_3 0.26,	SiO_2 2.90
No. 2—	PbO 0.3 CaO 0.6 ZnO 0.1	Al_2O_3 0.26,	SiO_2 2.90
No. 3—	Na_2O 0.3 CaO 0.6 ZnO 0.1	Al_2O_3 0.26,	SiO_2 2.90
No. 4—	BaO 0.3 CaO 0.6 ZnO 0.1	Al_2O_3 0.26,	SiO_2 2.90
No. 5—	ZnO 0.3 CaO 0.6 SnO 0.1	Al_2O_3 0.26,	SiO_2 2.80 Sb_2O_3 0.10
No. 6—	Na_2O 0.3 CaO 0.6 ZnO 0.1	Al_2O_3 0.26,	SiO_2 2.30 B_2O_3 0.60
No. 7—	MgO 0.3 CaO 0.6 ZnO 0.1	Al_2O_3 0.26,	SiO_2 2.90

$$\text{No. 8—}\quad \left.\begin{matrix} K_2O & 0.3 \\ CaO & 0.7 \end{matrix}\right\} Al_2O_3\ 0.50,\quad \left\{\begin{matrix} SiO_2 & 4.00 \\ B_2O_3 & 1.00 \end{matrix}\right.$$

$$\text{No. 9—}\quad \left.\begin{matrix} K_2O & 0.19 \\ CaO & 0.48 \\ ZnO & 0.13 \\ PbO & 0.20 \end{matrix}\right\} Al_2O_3\ 0.26,\quad \left\{\begin{matrix} SiO_2 & 2.68 \\ B_2O_3 & 0.30 \end{matrix}\right.$$

It will be noted that numbers 8 and 9 are the same in both series, number 8 being the glaze made from Cone 4 formula with one equivalent of boracic acid added, as produced by Dr. Hecht, of Berlin; and number 9 of both series being the glaze commonly known as the Trenton glaze. Both series were fired at Cone 6 and Cone 8, including the last two glazes without change, as above stated.

It will be noticed from the trials[1] that the No. 1 glazes, both Cone 6 and Cone 8, finished slightly dull, and that the colors are brighter with the Corfeld flux, than with the spinel.

The number 2 glazes, both Cone 6 and Cone 8, were fairly bright; colors brightest with the flux.

The number 3 glazes, both Cone 6 and Cone 8, were fairly bright; colors brightest with the flux

Number 4 glazes, both Cone 6 and Cone 8, were slightly dull; colors brightest with the flux.

Number 5 glazes, both Cone 6 and Cone 8, were dull and wrinkled; none of the colors were well developed.

Number 6 glazes, both Cone 6 and Cone 8, were dull or mat, and crazed on the vitreous tile; the colors were not well developed, especially the pink.

Number 7 glazes, both Cone 6 and Cone 8, were dull and wrinkled with the spinel, and a mat finish with the flux, pink particularly bad.

Number 8 glazes, both Cone 6 and Cone 8, were bright; colors all bright with the flux, except uranium; colors all bad with the spinel, except uranium.

Number 9 glazes, both Cone 6 and Cone 8, were bright; colors all bright with the flux, except pink; all bad with spinel.

1 The 1156 trials mentioned were mounted in rows and exposed during the session for the inspection of the society.

It will be noticed that very little difference, if any, exists in any of these trials between placing the color under the glaze, and placing it on the glaze, when fired in glost kilns.

It will be seen by the trials, that the differences in colors are more marked between the Corfeld flux and the spinel, than the differences produced by the different glazes, which would seem to justify the conclusion that the proper way to control the color and hue, would be by adopting the correct flux or hardening substance, and to regulate the tints and shades by the proportions of oxides and the mixture adopted.

By comparing the result of this series of trials, with the series of copper-green, stained-glaze trials, presented by Mr. Walker at our Cleveland meeting last year, it seems that the oxides, when reduced by fluxes or hardening substances, and used as underglaze colors, will produce a greater uniformity of color, than they do when they are actually calcined into fritt, which forms a part of stained or colored glazes.

Judging from the two series, it would seem necessary in underglaze work, to get, and permanently fix, the variety of colors, in the varying tints and shades of each, by calcining the oxides with the proper substances to produce the desired hue and tone. This would make a very interesting series of trials, and we are in need of data along this line.

It was with the hope of partially filling this gap, that the trials here presented were made, and this paper undertaken, and I must confess, that the results are far from being what I had originally hoped, and expected them to be. While plenty of underglaze colors are to be had, and numerous recipes for making same, yet all who are interested must certainly feel the want of any systematic record, along the lines above suggested.

If the effort made should be the means of stimulating further trials and research along this line, which should ultimately give us a means of permanently fixing any desired color in its varying tints and tones under good workable glazes, I will feel repaid for the effort.

DISCUSSION.

Mr. F. W. Walker: The amount of work which the members of this society are willing to take upon themselves in preparing papers for these meetings is astonishing. This paper of Mr. Stover's is an illustration of the point. Mr. Stover has made over eleven hundred trials, of which he presents about half of them here; so you can see what amount of work he has done. This is also another illustration of the great difficulty of discussing papers without opportunity for studying them in advance.

Dr. Edward Hart: I think what Mr. Stover needs more than anthing else, is some help. I would suggest in that line, as he has had difficulty in burning spinels, that he seek help from the electrical art. I undertook some work in the ceramic line myself, in the making of silica tubes. This was described to us at the Niagara meeting of the Electrical-Chemical Society last September. Since that time, Mr. H. H. Potter, of the Sawyer-Mann Electric Company, of New York, has been working along the same line, and not more than a month or two ago, I was in his laboratory, and found he had silica tubes there. A peculiar and interesting property of silica is the co-efficient of expansion, which is almost nil. You can heat them up, and plunge into water, and they will not crack. It struck me that I might make silica vessels, for boiling down chemicals. You can do almost anything in chemistry, if you only have something to do it with. The trouble is that some of these chemicals attack everything you put them in. I have already succeeded, I may say, in making a vessel about four inches in diameter of silica fused by the electric arc, and I would say to Mr. Stover that he can fuse his spinels by electric arc. The principal difficulty which I find is to keep the carbon out of the mixture. I presume there would be difficulty in getting these sesqui oxides fused without turning them into proto-oxides.

Mr. Karl Langenbeck: I would like to ask Mr. Stover whether any colors were applied faintly, so as to have an opportunity of judging of the relative solubility in the glaze of both spinel colors, and those made with Corfeld flux.

Mr. Stover: All were put on with a thin coat, then a thicker coat, then a couple of lines, to see what effect the thickness would have. In this one (indicating), fired at Cone eight, it stood very well in all thicknesses. In the majority of cases the colors fluxed with the Corfeld flux were best. I really consider the whole spinel series a failure, for the reason that they were not brought to a perfect degree of fusion.

Mr. Langenbeck: It has been a long time since I read the note of Seger in which he speaks of the making of underglaze colors on the spinel basis. My recollection is that his point was, that a color which approximates the character of a spinel, resists better the solvent action of the glaze. I do not think that preparing the color alone as a spinel is sufficient to make a complete underglaze of it. My impression is that the spinel should be formed as the chromo-genic body, which should be suitably fluxed. I think the trend of future work on this subject should be, not a comparison of the spinel body directly with an oxide flux, but a comparison of a spinel chromo-genic body suitably fluxed, with an agent to make it adhere to the glaze and develop its color, in comparison with a straight oxide flux.

THE NEW DEPARTMENT OF CLAY-WORKING AND CERAMICS AT RUTGERS COLLEGE.

By Cullen W. Parmelee, B. Sc., New Brunswick, N. J.

The establishment of this department is not Rutgers' introduction to usefulness in the field of ceramic industry. We interested in the old institution have a great deal of pride in the many notable services rendered the state by the late Dr. George H. Cook, a vice-president of the college. Conspicuous among his good works is the report on the New Jersey clays which he investigated as State Geologist, assisted by Dr. J. C. Smock, also of Rutgers. The report of 1878, needs no eulogy for the admirable manner in which the subject was treated.

Then should have followed the organization of a school of ceramics. It was not until the year 1901, however, that an effort was made to secure legislation toward this end. It was defeated, but the gentlemen interested believed thoroughly in the project, and were not to be discouraged. By diligence and generalship the recalcitrant legislators were brought into line and in 1902 passed the bill with but one dissenting vote. By the act $12,000 was appropriated for equipment and $2,500 for maintenance during the year 1903. Money for succeeding years must be annually voted.

In accordance with the terms of the law, the school performs the two-fold function of providing for instruction and for investigations, the director being charged with the duty of publishing in a convenient and accessible form the results of his researches.

The fiscal year of the state terminates on October 31st, and the balance of any appropriation unexpended at that time reverts to the state. You can readily understand that the four months in which I had to undertake and to complete the equipment and the organization were busy months, but very happily, I had the experience of Professor Orton and Professor Binns to draw upon. Permit me, to express my appreciation of their interest and helpfulness.

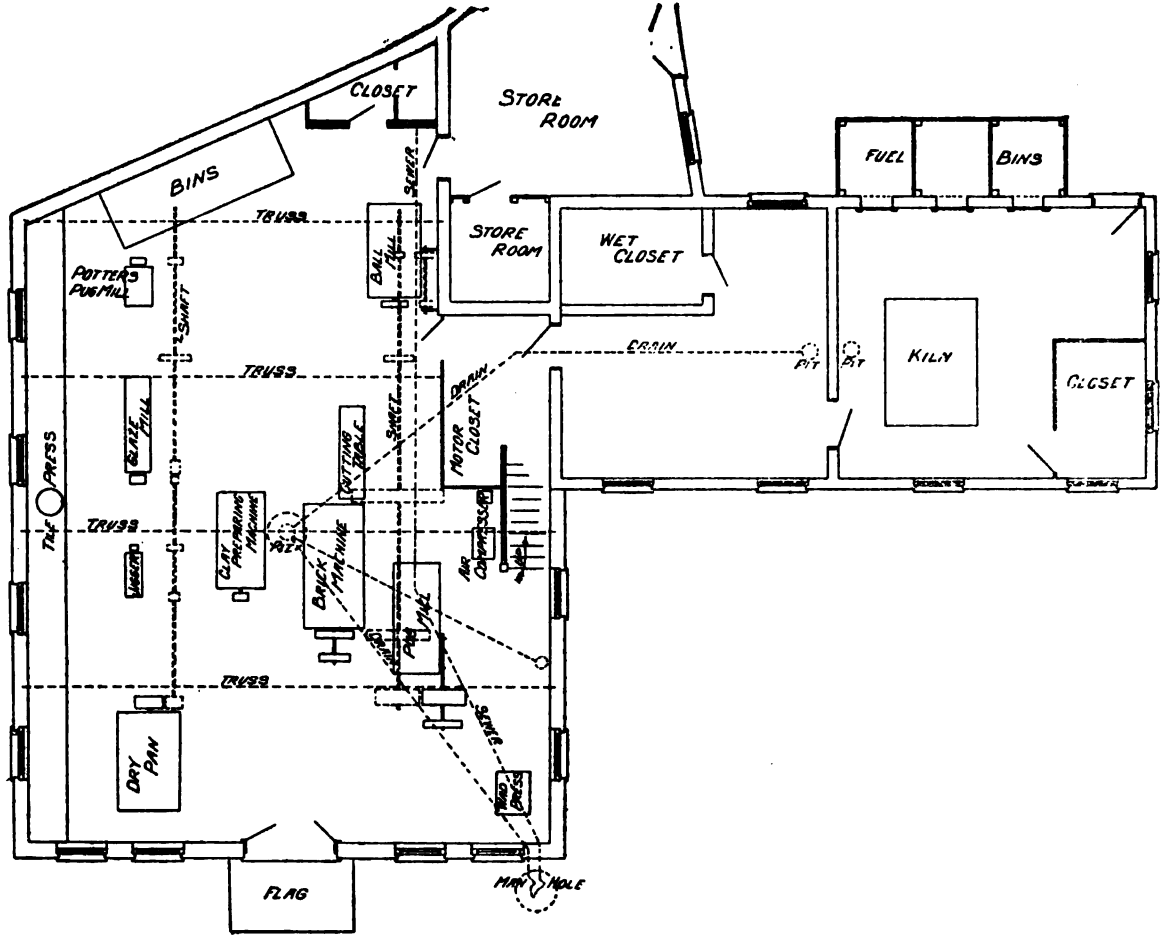
PLATE I. EXTERIOR VIEW OF THE MAIN WING.

Our first need was proper accommodations. There was no suitable place in any of the college buildings, and with one small appropriation we could not afford to erect a fine large new structure. There was one college property however, a two-story brick building, unused, but which formerly served in the humble capacity of a barn. This had six small rooms, in all a floor space of about 1600 square feet. We accepted this as neuclus and added thereto an equivalent number of square feet in the form of a one-story brick addition.

This new portion serves for our machinery room. It has a good cement floor, pitching slightly toward the center, where there is a grating and pit with sewer connections. On one side of this room stretches a work bench forty feet long. Numerous windows furnish good light; ample gas and water supply is provided, also toilet facilities. Connected with this room is the store room which conveniently opens on a court with alley connection to the street. Another adjoining room contains one wet closet which serves in lieu of a cellar. This is an inclosure 6x9 with cement floor, soft brick walls and terra cotta lumber ceiling and an iron door. The interior can readily be made wet by a hose, and when closed will retain its dampness for a long period. The interior is lined with racks for shelving.

Next is the kiln room. This is at the end of the old building and separated by a brick partition wall. It is only 20x27, and of course our fuel bins had to go outside, ports being cut through the wall to give access to the fuel. One kiln has been built and there is room for another small one. In its place temporarily rests a wooden closet 6x9, provided with shelves and designed for the storing of ware for drying preparatory to firing.

In the second floor of the old portion, two rooms are used, one for a library, museum and class room. Its walls are lined with cases having sliding glazed doors. This affords space for the display of our ceramic collection, that is, for the smaller pieces. In the workshop, a shelf has been built on three sides of the room at about eight feet above the floor, this is intended for the large coarser wares. In the small room



CLOSET
STORE ROOM
BINS
FUEL
BINS
SEWER
TRUSS
STORE ROOM
WET CLOSET
POTTERS PUG MILL
BALL MILL
SHAFT
DRAIN
PIT
PIT
KILN
CLOSET
TRUSS
DRAIN
GLAZE MILL
SHAFT
MOTOR CLOSET
CUTTING TABLE
TILE PRESS
TRUSS
CLAY PREPARING MACHINE
PIT
BRICK MACHINE
AIR COMPRESSOR
JIGGER
PUG MILL
TRUSS
SEWER
DRY PAN
WAD PRESS
FLAG
MAN HOLE

we have provided shelving for our literature on ceramic topics, a considerable collection being already gathered.

The director's office and his laboratory is also on the second floor. As the ceilings were low and under a flat tin roof, it was decided to light and ventilate this room by means of an eight foot square iron and glass skylight with two movable transoms on each side. The floor is one inch asphaltum laid on the old flooring. This has the advantage of being waterproof and practically fireproof. There is the usual equipment of working table, sink, hood and wall case for supplies.

Metal ceilings have been used where ceilings had to be provided. They are clean, attractive in appearance, and may readily be kept so. Cement has been used for flooring on the ground floors excepting in the kiln rooms, which is clay.

The gas supply is brought into the building by an 1½ inch pipe, and none of the piping is less than ¾ inch. This is of course a generous provision for such a small building, but it insures an adequate supply.

In securing the equipment I found the manufacturers of machinery very kindly disposed toward the project. In every case the machinery was purchased at special prices, and in many instances at figures which made them practically gifts.

Power for running our machinery is furnished by a 30-horse power, 4-pole 250 volt motor furnished by the General Electric Company. This power is transmitted by two lines of shafting, each 33 feet long, with self-oiling adjustable hangers, and carrying, with two exceptions split steel pulleys.

The machinery consists of brick machine outfit, potter's and tile maker's appliances. Its arrangement can be seen on the plan. The brick-making machinery consists of three pieces, all furnished by the American Clay-Working Machinery Company. They are an auger brick machine of a capacity of 20,000 brick per day, a five-foot horizontal pug mill and a side-board delivery cutting table. All these are large pieces, requiring much power, a considerable quantity of raw material and are really too big for an experimental

plant, but are the smallest practicable machine obtainable.

The potter's outfit was furnished by the Crossly Manufacturing Company, of Trenton, N. J. It consists of:

A four-foot grogmill with blank plates which may be used when run as a wet-pan. The mullers have a five inch face.

A variable speed jigger and pull down and throwing wheel combined. On this, pieces as large as a ten inch jardiniere may be made.

A four-jar glaze mill, the jars having a capacity of about two gallons each. They are made of a porcelain body and furnished with flint pebbles.

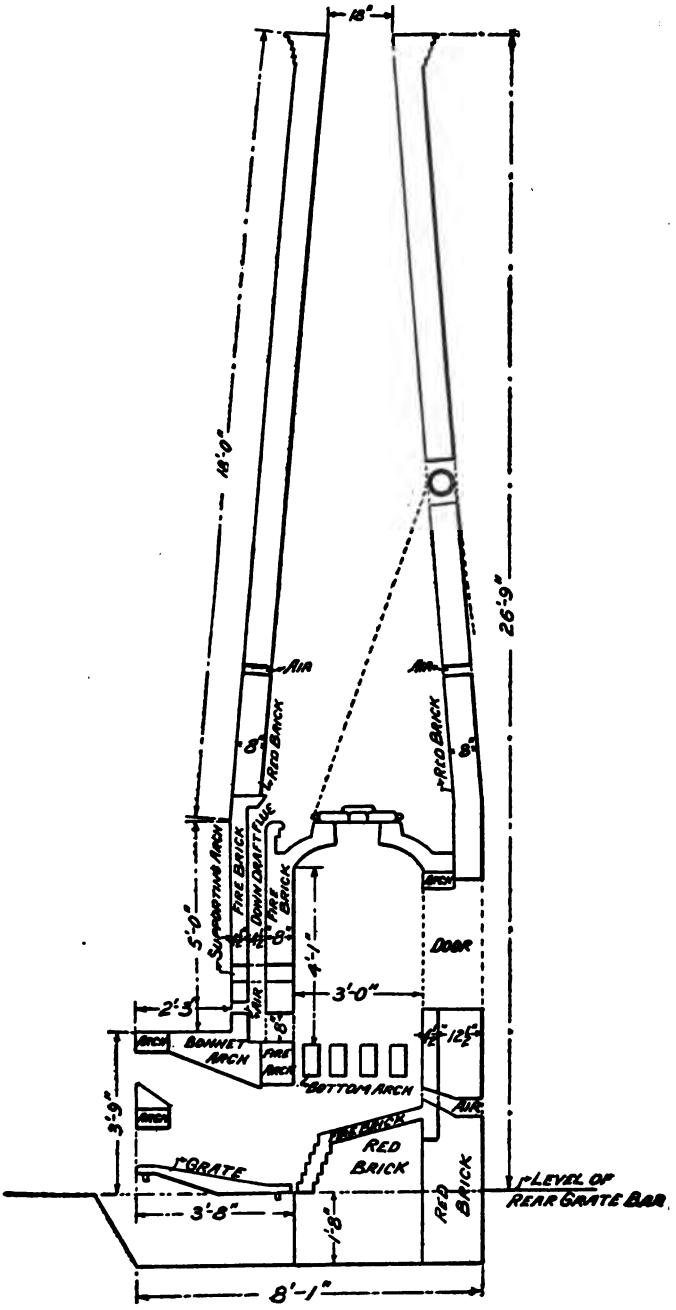
A twelve-inch potter's pug mill.

A clay preparing machine which is an attractive and compact arrangement of blunger, agitator, lawn screen, slip pump and filter press. It has a capacity of from 500 to 1,000 pounds a day. There are 20 plates in the filter press, each 9 inches square, and the press can be filled in about thirty-five minutes.

There are also a small tile press, a hand jigger, a mould maker's jigger, a wad mill and a stilt press. For grinding large quantities of material we have a large ball mill with a one piece lining 2 inches thick. It has internal dimensions 3 feet by 2 feet.

For use with a Deville furnace, we have a Crowell rotary pressure blower which is capable of furnishing pressure of ten pounds to the square inch.

We have provided only one kiln, as our present needs and resources limit our expenditures. It is designed however, to do the work of both the up-draught and down-draught, which is provided for in this wise: The kiln space is three feet square by four feet high; the floor is made by the tops of four arches which spring from the sides at the level of the grate bars. When used as an up-draught, the flames and the gases arising from the fire-box, pass upward through the spaces between the arches, up through the kiln space and into the stack, through openings in the crown. For down-draught purposes, these openings in the crown are closed. Quarries of the same length but lacking four inches


18"
18'-0"
26'-9"
AIR
AIR
8"
RED BRICK
RED BRICK
8"
SUPPORTING ARCH
FIRE BRICK
DOWN DRAFT FLUE
FIRE BRICK
8"
5'-0"
4'-1"
3'-0"
DOOR
2'-3"
AIR
8"
BRICK
BONNET ARCH
FIRE BRICK
BOTTOM ARCH
12½"
12½"
AIR
3'-9"
BRICK
FIRE BRICK
RED BRICK
GRATE
LEVEL OF REAR GRATE BAR
3'-8"
1'-8"
RED BRICK
8'-1"

of the width, are laid on the floor of the kiln space. Quarries are placed vertically near the side wall and two inches therefrom, leaning against projecting bricks. To prevent the falling backward of these, provision has been made for the insertion of bricks to support these vertical quarries parallel to the side walls. Having done this, we have provided flues against the side walls. The flames and hot gases rising through these are deflected by the crown and drawn into a flue opening in the end wall of the kiln near the floor. This flue empties into the stack above the crown.

The fire mouth is designed for soft coal. The grate-bars have an area of 880 square inches. They are three feet eight inches long, and have a drop of about five inches.

The damper is a fire-clay disc hooped and hinged to a ring about the crown. It is controlled by means of a chain which runs through a pulley in the side of the stack and this opens and closes the one foot circular opening in the crown.

The kiln is eight feet one inch long, five feet eleven inches wide. From floor to top of hob is three feet nine inches; from top of hob to shoulder is five feet; from shoulder to top of stack is eighteen feet.

Unfortunately I cannot give any results of firing owing to the fact that we have not had opportunity to give it a thorough test.

We have also a Deville furnace, a gas furnace for fritting, and a Seger test kiln improved by Heincke. We are provided with an electric pyrometer and Seger cones.

Two courses of study have been arranged. One covering a period of four years, the other, a short course. In the former the student will have a liberal training in the related sciences with especial emphasis placed upon general and analytical chemistry. During the latter two years he specializes on ceramics. General and economic geology, raw materials, bodies, glazes, drying and firing are topics studied in the lecture room, and experimented upon in the laboratory.

The short course is offered for the practical man who has had experience in clay-working and who recognizes the

PLATE 2.—DIRECTOR'S OFFICE.

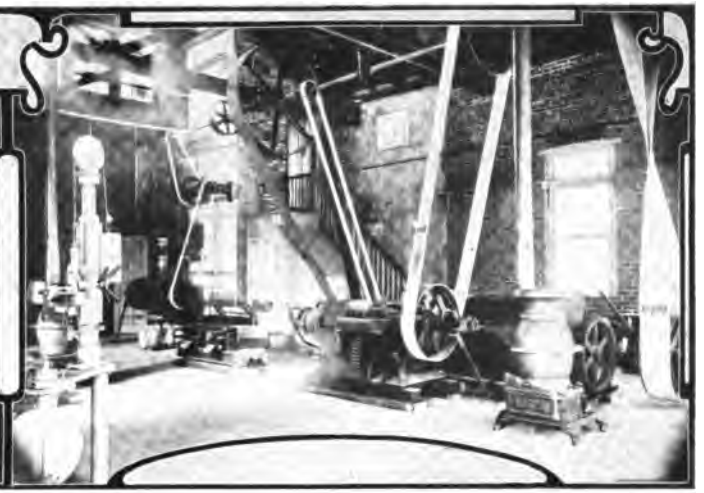


PLATE 3.—MACHINE ROOM.

value of scientific training, but is unable to undertake the full course.

We have at present seven students, and the indications for the future are bright.

DISCUSSION.

The Chair: I am sorry Mr. Bloomfield could not have been here, to have heard Professor Parmelee's paper. We really owe a great deal to our friend Bloomfield, for he certainly worked hard and persistently for three years, to the end that New Jersey should have a ceramic school. His work with the legislature was invaluable, and he really is the father of the school.

Professor Orton: This society undoubtedly recollects the part it had in the establishment of this plant for teaching ceramics. We lent our influence, whether it was great or small, toward pushing the bill through the New Jersey legislature last winter. For myself, I am much gratified to see the description of other ceramic schools becoming a part of our records of progress. With a multiplication of facilities along this line such as is surely coming, we can't help producing a class of men which will aid materially in the work of our society and in the greater and larger end to which this society stands pledged, that of building up the ceramic industries of this country. There is certainly no state in which a ceramic school could more suitably be started than New Jersey, which stands well toward the top among the clay-working states of the Union. I think the society should welcome this addition to the list with all hail.

Mr. Langenbeck: I do not know that I can say anything in addition to what has been said, but I have been much interested in the movement, and felt very much gratified at the energy with which Professor Parmelee has taken hold of the work. Ordinarily, they do not feel quite certain yet as to the benefit of the application of science in the pottery industry, but I am happy to say that I have heard many cordial expressions of interest in this case from potters of Trenton.

THE INFLUENCE OF MAGNESIA ON CLAY.

By Adolph E. Hottinger, Chicago, Ill.

Under the above title, Dr. Mäckler read a paper before the German Society for Clay, Cement and Lime industries. Being very much interested in this paper, I made some experiments on the subject and give some results that I obtained, and also some of the original.

A brief summary of the paper in question shows that the action of magnesia on clays is a subject to which little attention has been paid; but, from the results obtained, it is shown that exceedingly valuable properties are given to clays carrying this element naturally or into which it is introduced.

The characteristics of clays of this nature are that they may be made into ware of extreme length and with very thin walls, which may be very nearly vitrified, and still be kept perfectly straight and true. Also, that where exact shapes are required, especially in vitreous ware, clays of this nature are valuable.

The reason of this is in the melting and vitrification point of clays of this nature. Lime and its influence on clays is well understood, and it is well known how near together the vitrifying and melting points lie. Clays carrying magnesia are very different in their behavior; and the melting and vitrifying points are farther apart, and such clays consequently can be made into vitreous ware, without great care in burning.

Another interesting fact developed is that kaolin with magnesia gives dense bodies at comparatively low temperatures, in distinction from lime, which has but little action at the same temperature.

Some trials made on this line show interesting results. Mixtures were made as follows, and the absorption taken of trials burned at cones one and five:

Number of Test.	Mixtures.	Absorption.	
		Cone 1.	Cone 5.
A 1.	Kaolin 100, Whiting 10	34.7	30.2
A 2.	Kaolin 100, Magnesite 10	25.2	17.1
A 3.	Kaolin, without additions	25.5	24.7

I also quote mixtures as made by Dr. Mäckler:

Number of Test.	Mixtures.	Absorption.	
		Cone 1.	Cone 5.
J.	Kaolin 100, Marble 25	24.8	23.9
K.	Kaolin 100, Marble 12½	24.7	24.1
L.	Kaolin 100, Magnesite 21	16.4	18.9
M.	Kaolin 100, Magnesite 10½	18.8	15.9
	Zettlitz Kaolin, with addition	25.9	18.3

From the last experiment it will be seen that Dr. Mäckler used the lime and magnesia in equivalent parts, and by observing the mixtures M and K, at Cone 5, having equal equivalents of lime and magnesia, the latter lowered the absorption of the trial M 8.2 per cent. over the trial K. It is also noticed that lime in J and K lowered the absorption over the original kaolin somewhat at Cone 1, while at Cone 5 the absorption is 5.6 per cent. greater than that of the kaolin.

I was not able to get results anywhere near the lime mixtures, as given, but approached closely to the magnesia experiments. This is probably due to a difference in mixing, and probably in material used. Dr. Mäckler's mixings were all made in a slip state, and were ground in a mill, while I made mine dry, first rubbing them together in a mortar and afterward sifting twice. This would not give as intimate a mixture as the original; but the result shows the advantage of the use of magnesia.

To get an idea of what advantage magnesia might be in a porcelain mixture, I made some trials with lime, magnesia and also dolomite, containing 50.28 per cent. calcium carbonate, and 40 per cent. magnesium carbonate, etc. The idea was to use dolomite as a source of magnesia.

For the porcelain body I used the formula of the Berlin porcelain as given by Seger:

Clay Substance,	55.
Feldspar, -	21.2
Quartz, - -	23.5

The mixtures are as follows:

Number of Test.	Mixture.	Absorption.	
		Cone 1.	Cone 5.
B 1	Porcelain Body 100, Whiting 10,.....	26.2	24.9
B 2	" " 100, " 5,.....	22.0	11.8
B 3	" " 100, Dolomite 10,.....	20.2	12.4
B 4	" " 100, " 5,.....	19.1	10.8
B 5	" " 100, Magnesite 10,.....	18.8	9.9
B 6	" " 100, " 5,.....	13.2	6.0
.....	Porcelain Body, without addition.....	22.5	14.4

B 6, of this series, at Cone 5, proved to be the best, being a very hard, close body, which cannot be scratched with a hard steel point. B 3 and 4, containing the dolomite, are an improvement over the whiting, but cannot equal the magnesia mixtures.

An interesting fact developed by the experiment is that, used in equivalent amounts, magnesia will not give the same result as lime.

As an instance of this, the Cone 4 mixture was taken, consisting of 0.3 equivalents Feldspar, 0.7 calcium-carbonate, 0.2 kaolin, 1.8 quartz. In this mixture, the lime was replaced by an equivalent of magnesia, and the mixture fired to Cone 1. A cone made from the lime mixture was vitrified, while the magnesia cone was still porous. Firing to Cone 5, the magnesia cone is vitrified, but still remained standing, and showed not the least sign of melting, the corners remaining perfectly sharp. Not having any higher cones in the kiln during this experiment, the fire was raised until Cone 4 was melted to a nearly clear glass, and when this point was reached the magnesia cone had gone down.

A series of experiments with a shale of the following analysis:

Silica, - - - - -	62.66
Alumina, - - - - -	21.26
Ferric Oxide, - - -	3.50
Calcium Oxide, - - -	0.92
Magnesium Oxide, - -	0.54
Alkalies, - - - - -	4.60
Volatile and loss, on ignition,	6.44
	99.92

also showed some interesting results, in particular the difference in the melting points of the shale, with the lime and magnesia added.

The following mixtures were made as follows:

No. of Test.	Mixture.	Absorption.			
		Cone 05.	Cone 1.	Cone 3.	Cone 5.
C 1	Shale 100, Whiting 25,	21.9,	Melted,	Melted,	Melted,
C 2	Shale 100, Whiting 12½,	20.4,	Vitrified	Melted,	Melted,
C 3	Shale 100, Magnesite 21,	32.8,	7.3	Vitrified	Shape retained,
C 4	Shale 100, Magnesite 10½,	28.4,	0.32	Vitrified	Slightly swelled,
C 5	Shale 100, Dolomite 22,	22.4,	11.5	Parti'lly melted,	Melted,
C 6	Shale 100, Dolomite 11,	17.7,	12.1	Blister'd	Melted,
	Shale, without additions.	12.0,	0.78	Blister'd	

It is noted from the above that C 4 is practically vitrified at Cone 1, and does not show signs of distress until Cone 5, when it swelled slightly.

The dolomite mixture did not come up to expectations and showed less range in their melting and vitrification points than the lime mixtures, as at Cone 1 they are still very porous, while at Cone 3 they had started melting, and had lost shape.

DISCUSSION.

Mr. Hottinger: I did not intend this as a paper, but merely as a note on a line of experiments which I thought would be of interest.

Mr. E. C. Stover: I think Mr. Hottinger's paper is one which needs no apology. I am glad to see that he has

* Cer.

reached out a little into the field of our German friends, and has taken up a line of work suggested thereby, and brought the results here. We have enough here to work out ourselves, yet we cannot offord to ignore the work of the other world. If those who read the German technical papers do not do this work, the rest of us will lose much that we ought to know. This question of finding a material to operate as a flux in a vitreous body, which will widen the range between the point of actual vitrification and the point of viscosity, is a point we are all interested in. As shown by Mr. Hottinger, with whiting as a flux, these points lie close together, and a flux which will widen these points will be important to anyone working in vitrified bodies.

Mr. Hottinger: That is illustrated by taking this shale and adding the magnesite to it. At Cone 1, the cone with the whiting melted. Taking the same mixture with magnesite, it showed no signs of melting until Cone 5 was reached, and under the experiments shown in the Germon paper, not even at Cone 7 did it show signs of melting, though it swelled slightly, and it took a good deal beyond that to bring it to a glass. Another point shown by the German paper is that in porcelain or vitreous wares, where exact shapes are wanted—for instance, electric porcelain—magnesite plays an important part. You will note that in the porcelain mixture it did not show swelling. In the shale mixture, firtd above the vitrification point of the shale, there was slight swelling.

The Chair: In the porcelain mixture, does it show swelling at a certain point?

Mr. Hottinger: I did not try it for that. I tried to show what effect magnesite has upon the mixture at Cone 5.

The Chair: That touches right on the point I am going to bring up later as regards this swelling for electric insulators, (exhibiting samples). Many of the electrical porcelain pieces as now made are quite intricate, it being necessary for them to fit expensive metal parts, and they must necessarily be exact. I have brought with me two samples which show that point of swelling. Such pieces which are slightly blistered and swelled must be rejected, because they will not

fit the metal portion of the apparatus which they are made to complete.

Professor Edward Orton, Jr.: As I understand it, in the paper presented by Mr. Hottinger, one of the chief points of value is the demonstration of the fact that magnesite as a flux will enable us to get vitrification at actually lower temperatures. So it not only widens the range of vitrification, but enables us to use lower temperatures than otherwise.

FURTHER STUDIES ON WHITE BRISTOL GLAZES.

By Ross C. Purdy, Columbus, O.

In as much as formula I:

$$\left.\begin{array}{l}0.5\ K_2O\\0.4\ ZnO\\0.1\ CaO\end{array}\right\}\ 0.55\ Al_2O_3\ \left\{\begin{array}{l}8.30\ SiO_2\\.01\ Na_2B_4O_7\end{array}\right.$$

as developed in my article on "Stoneware Glazes" before this society last year, was a surprise to those familiar with the formulae of Bristol Glazes most generally used in the stoneware industries today, because of the relatively higher equivalent content of potash and alumina and low ratio existing between the alumina and silica, it is the object of this paper to prove or disprove the points in question.

From the following formulae which show the composition of the glazes actually in use among representative stoneware factories, it will be noted that the majority of them do not agree with the above formulae I in the use of such high equivalents of either potash or alumina.

TABLE I.

	K_2O	CaO	ZnO	Al_2O_3	SiO_2
II	0.237	0.327	0.433	0.347	2.275
III	0.225	0.388	0.386	0.356	3.450
IV	0.327	0.300	0.373	0.366	2.540
V	0.208	0.434	0.358	0.332	2.780
VI	0.403	0.201	0.395	0.473	2.960
VII	0.377	0.180	0.444	0.516	2.840
VIII	0.278	0.194	0 527	0.403	1.920
IX	0.342	0 306	0.322	0.418	2.220
X	0.199	0.461	0.340	0.306	2.630

It is not a surprise then to learn that the practical potters who use glazes of these compositions should look askance at such a radical change as advised in formulae I. The fact is, however, that the two giving the best satisfaction are VI and VII. They are the best in regards to opacity, gloss and general working qualities; thus indicating

that better results *can* be obtained by use of a higher equivalent of potash and alumina than *is* the general practice.

It must be admitted, however, that on a commercial basis, glaze No. I did not prove satisfactory. The fault, however, did not lie in the use of a too high content of alumina. It had a good gloss and was very opaque; so, from a chemical standpoint, it was correct. But its physical properties were such as to make it very unsatisfactory as a glaze. It crawled very badly. In fact, it drew up into large globules. It was impossible to handle the ware after it had been dipped because large patches of the glaze would scale off.

Further experiments were made with glaze I as a basis, to discover and remedy the trouble.

It is not necessary at this time to give all the experiments made, because the cause and solution of the trouble can be illustrated by giving only the formulae of the best glazes deduced.

TABLE II.

	K_2O	CaO	ZnO	Al_2O_3	SiO_2
XI	0.405	0.194	0.405	0.505	2.845
XII	0.450	0.150	0.400	0.600	3.250
XIII	0.370	0.230	0.400	0.500	2.900
XIV	0.375	0.225	0.400	0.500	3.125

These glazes were used on a commercial scale under similar conditions of firing and on the same clay, giving satisfaction in all respects.

Two facts are to be observed by comparing glazes XI, XII, XIII and XIV with I.

1st. That it is possible to use as high as 0.55 equivalents of alumina.

2nd. That it is not feasible or possible in the case in question, for physical reasons, as well as chemical, to use so high an equivalent of feldspar; which means, as a sequence the lowering of the equivalent of potash.

The formulae of several glazes which had crawled have been given* Their equivalents of alumina ranged from

*Vol. IV Trans. A. C. S., p. 66.

0.55 to 0.75. The deduction was drawn that the use of such high equivalents of alumina and consequently large equivalents of clay was the prime cause of the crawling. But in the light of the present experience, I now beg to modify that statement.

In all these glazes, except two which were thus cited, the feldspar content ranged from 0.50 to 0.55 equivalents, and in the two exceptions noted, the equivalent of ZnO was above 0.4, which in itself is a prolific source of crawling.

It is seen then, that glaze I crawled so badly because the per cent. of feldspar was too high, thus creating physical difficulties which could not be counterbalanced by the supposed increase in fusibility obtained from the use of the larger amount of feldspar. The physical limitations from the addition of large quantities of feldspar arising in a Bristol Glaze are as positive as the chemical limitations and of equal importance.

In the light of formulae XI, XII, XIII and XIV, it is seen that 0.5 equivalent of feldspar is not necessary for the proper maturity at Cone 7 of a Bristol glaze having 0.55 equivalent alumina, which is in contradiction to statements made by the writer last year.[1] In fact, the use of 0.40 feldspar make a more fusible mix than does 0.5 equivalent.

Clay workers generally consider that refractoriness is *always* in proportion to the alumina, but this is not true as will be shown.

Seger[2] made into cones the following mixtures, by compounding 1.0 equivalent of feldspar successively with 1.0, 2.0 and 3.0 equivalents of carbonate of calcium.

$$\text{No. 1—} \left.\begin{matrix} 0.50\ K_2O \\ 0.50\ CaO \end{matrix}\right\} 0.50\ Al_2O_3,\ 3.0\ SiO_2$$

$$\text{No. 2—} \left.\begin{matrix} 0.33\ K_2O \\ 0.66\ CaO \end{matrix}\right\} 0.33\ Al_2O_3,\ 2.0\ SiO_2$$

$$\text{No. 3—} \left.\begin{matrix} 0.25\ K_2O \\ 0.75\ CaO \end{matrix}\right\} 0.25\ Al_2O_3,\ 1.5\ SiO_2$$

He was surprised to find, notwithstanding the fact that the RO's were increased by decreasing the alumina and

1 Transactions of A. C. S., Vol. IV. Page 69.
2 Translation of the Collected Writings of Herman Seger, Vol. I, Page 222.

silica, that each of the mixtures were more refractory than the pure feldspar.

Mixtures of clay with feldspar, and flint with feldspar similarly increased its refractoriness. But he found that when calcium carbonate and clay together were added, the result was quite different.

From the tables of Seger's trials the following facts can be noted:[1]

1st. That each of the several mixtures of feldspar, quartz and marble adopted as a basis of his several series, were made more fusible by the addition of 0.5 equivalent of kaolin.

2nd. That his notes on the same experiments indicate that in all mixtures where the quartz content is not more than six equivalents, even 1.0 equivalent of kaolin made them but a trifle, if any, less fusible than when only 0.5 equivalent of kaolin was added.

3rd. That in series IV which has for its base

$$\left.\begin{matrix}0.25\ K_2O\\0.75\ CaO\end{matrix}\right\}\ 0.25\ Al_2O_3,\ 3.0\ SiO_2$$

the addition of 1.0 equivalent of kaolin actually makes it more fusible than where 0.5 equivalent of kaolin is added.

4th. That in the most fusible mixtures, the content of alumina ranged from 0.5 to 0.75 and never fell below 0.5.

5th. That "it appears distinctly that neither the glazes lowest in alumina are really the most fusible ones, nor those with a higher content of alkali, but that for low fusibility there must evidently be a *definite proportion between* the *alumina* and *fluxes.*"

In the discussions under this same topic last year, formulae and samples were exhibited, illustrating this same fact of the increased fusibility incurred by the addition of 0.05 equivalent of clay to mixtures which contained no clay but which had 0.5 equivalent alumina.

Professor Wheeler[2] quotes Dana's table showing the fusibility of feldspars.

1 Translation of the Collected Writings of Herman Seger, Vol. I, Pages 284-286 inclusive.

2 Missouri Geological Survey. Vol. XI, Page 147.

TABLE III.

Name.	Silicate Degree.	SiO_2.	Al_2O_3.	K_2O.	Na_2O.	CaO.	FeO.	H_2O.	Total RO.	Fusibility.
Orthoclase	Tri-	64.7	18.4	16.9	.....	.....	.....	.....	16.9	5.0
Albite	Tri-	68.7	19.5	.....	11.8	.....	.....	.....	11.8	4.0
Oligoclase .	{ ⅔ Tri-.... ⅓ Bi-..... }	61.0	24.0	2.0	9.0	3.0	.....	.....	14.0	3.5
Labradorite	Sesqui-	54.0	29.0	.....	5.0	11.0	1.0	.....	17.0	3.0
Anorthite .	Mono-.....	43.2	36.7	.....	.....	20.1	.....	.....	20.1	5.0
Kaolin ...	{ ⅔ Mono-.. ⅓ Bi-.... . }	46.3	39.8	.....	.....	.....	.....	13.9	.....	7.0

"From a study of this table it will be seen that the least silicious feldspar, anorthite, is the most infusible, while labradorite, one of the most aluminous, is the most fusible, which is the reverse of what would be expected from Bischof's formula; the latter feldspar, moreover, has 17 per cent of RO, as has also the least fusible feldspar, orthoclase, which shows that it is not a mere question as to the amount of RO or protoxide bases present."

The degree of fusibility or refractoriness caused by the alumina is governed 1st, by the greater or less reaction between the incorporated ingredients; 2nd, by the readiness of the alumina bearing ingredients to fuse; 3rd, by the readiness with which the most fusible element will become a liquid in which the other ingredients are more readily brought into combination with each other; 4th, by the physical condition of the ingredients.

There are at least two plausible reasons why an addition of 0.05 and 0.1 equivalent of kaolin to the Seger mixtures before quoted, and those given in my paper last year, makes the resultant mixture more fusible.

1. We know that clay when burned just sufficiently to accomplish dehydration becomes a Puzzolain material, *ie.*, when mixed with CaO, it will react with the lime making a Puzzolain cement, as do the Puzzolain slags and stones. This indicates that the anhydrous silicate of alumina formed, is in such a condition of chemical activity that it is capable

of uniting with other elements, especially calcium oxide, at a lower heat.

2. Carbonate of calcium mixed with clay begins to lose its carbon di-oxide below 800°C, as shown by the following data of heat of hydration:—

This experiment was made by Mr. A. V. Bleininger, on a mixture of carbonate of calcium plus 20 per cent. kaolin. The point when calcium oxide was found free in the mixture was noted by the heat evolved when it took on chemical water, forming calcium hydrooxide as in ordinary lime slaking, the heat evolved being measured in the calorimeter.

Degrees.	Calories.
500°	——
700°	——
800°	32.13
900°	126.43
1000°	128.52

This shows that combination between clay substance and caustic lime takes place below 800°, as at 800° we already have a considerable excess of calcium oxide as shown by heat evolved.

Bleininger also proved that kaolin is perfectly decomposed by caustic lime at 800° as was shown by all of its silica being removed by sodium cabonate solution.

Bleininger's experiments also showed that calcium carbonate when mixed with feldspar, begins to lose its CO_2 at 900°, and with free quartz at 925°. Thus it is shown that when calcium carbonate is mixed with clay, the clay becomes ready for chemical activity at a considerably lower heat than does the feldspar, due no doubt to the SiO_2 set free in a condition in which it most readily offers itself for combination with other elements.

Seger says[1] "Mixing substances ever so intimately will not be sufficient to induce combination. Chemical processes rather presuppose an unrestricted motion of the molecules, and never take place between two solid substances; one of these, at least, must be in a liquid or gaseous state. Hence, two solid substances will not produce chemical compounds,

1 Translation of the Collected Writings of Herman Seger, Vol. I, page 491.

unless a liquid is present that will dissolve one or both of them, and thus establish the proper contact."

This law of chemical reaction produced by the intermediation of a liquid or molten magma is not, in a ceramic mixture, *always* dependent *primarily* upon the fusibility of any one or more ingredients if taken alone. As has been shown in the case of clay and calcium carbonate, the reaction is facilitated or hastened by the chemical activity of the dehydrated clay.

Feldspar when fused, or in a liquid state, is capable of dissolving silica. A. Buenzli, (1876)[2] found with the aid of the microscope that at high temperatures, feldspar will dissolve a quantity of silica and that the amount of dissolved silica increased with the rise in temperature.

Then, if the carbonate of calcium has reacted on the clay at 800°, reducing it in a condition of chemical activity, and the feldspar has been reacted on at 900° and free quartz at 925°, it is plain to see that there is considerable silica in such a condition as to combine readily, forming silicates of the various bases present.

Fusion then, would be greatly accelerated by the action of the calcium oxide on the other ingredients; the more the calcium carbonate added (up to a certain limit) the earlier the fusion.

The general law governing this fluxing action is explicitly stated by R. T. Stull[2].

"Fusion or decomposition (of a fluxing mineral)may be brought about at lower temperature by the action of other substances upon it. This is generally the case of a flux acting upon a flux. In this way, a high fire flux may be made to exert its fluxing power at a temperature far below that at which it would otherwise occur."

It will be shown in the experiments to be given later, that while, in a Bristol glaze mixture containing no clay, the fusibility of the glaze is increased by increase of feldspar, greater fusibility is obtained by the introduction of clay up to a certain equivalent; and also that this increased fusibility

1 Translation of the Collected Writings of Herman Seger, Vol. I, page 507.
2 Trans-American Ceramic Society, Volume IV, page 255.

due to the addition of clay, is in proportion to the amount of calcium oxide present, thus showing that the reaction between the calcium carbonate and clay (two most refractory ingredients) does greatly facilitate and hasten the fusion, more so than would further addition of feldspar; also that by virtue of this reaction between the calcium carbonate and clay, glazes having 0.45 or 0.55 Eqv. of Al_2O_3 are the most fusible ones, providing the feldspar does not exceed 0.4 Eqv. This brings glazes carrying .55 Eqv. Al_2O_3 within the required range of fusibility of the average stoneware potter.

The extent to which the fusibility of the aluminous ingredient controls the fusibility of the whole mixture, is illustrated very clearly in cement manufacture. In all cement mixtures the chemical composition varies within very narrow limits. This narrow limit of variation in composition, according to all refractory quotients, and to popular opinion as well, should bring the finishing temperature of the clinker formation within very narrow limits of variation. Such, however, is not the case. It varies with the fusibility of the clay employed, the amount of clay substance proper, the fineness of sub-division of quartz, the presence or absence of feldspathic minerals, and the character of the carbonate calcium, whether crystaline or amorphous.

The fusion of a glaze mixture is not a function of temperature alone, but also of the manner of firing, the time given, and the form of the furnace, etc., as shown by Hecht[1].

Bristol glaze, No. XI, when burned on a green stoneware clay body in a majolica biscuit kiln, with gas, where the temperature was raised as rapidly as possible in 48 hours, fused to a beautiful white Bristol glaze at Cone 4. One of the managers of the company informed the writer later that in the glost fire at Cone 2, burned in 20 hours, this same glaze was a beautiful white glaze, perfectly matured. But when it was tried in a stoneware kiln, where the temperature was raised slowly with coal, it required Cone 7 to make it a good bright glaze.

The fuel used, whether gas or coal, makes a very appreciable difference in the results indirectly; but time taken to

1 Thonindustrie Zeitung, 1895, page 808.

mature the body and glaze, certainly has a direct and positive influence. Especially is this so in a Bristol glaze when zinc oxide is used in quantities ranging from 9.25 per cent. to 15 per cent. of the total batch weight.

In a fused Bristol glaze properly burned, the zinc is undoubtedly in three states (a) combined, (b) dissolved, (c) suspended in the glaze matrix The effect on the fusibilty of the glaze with the zinc in these various conditions, is obvious. In the first condition, its fluxing effect must be direct; in the second, slight, if any; and in the third, it cannot have any effect whatever.

From practical experience, the writer has learned that the longer the period of firing after fusion begins, the less opaque the Bristol glaze will be, showing that in the longer burn, more of the zinc is taken into the solution and combination.

Whether a Bristol glaze attacks the body sufficiently to cause a difference in the fusibility of the glaze on different bodies and in different lengths of time, I do not know.

S. Geijsbeek[1] cites a case where a glaze, the composition of which he did not know, but judged to be a zinc boro-lime glaze, did attack the white engobe to such an extent as to make the body of the ware plainly visible. But he says later, "Glazes generally used in stoneware potteries, high in alumina and silicic acid are usually well adapted for engobe purposes.

Langenbeck[2] has given the analysis of a glaze which had been used over an engobe; its chemical formula being

$$\left.\begin{matrix}0.26\ K_2O\\0.74\ CaO\end{matrix}\right\}1.04\ Al_2O_3,\ 5.76\ SiO_2$$

He says, "In the severe fire (Cone 9 or 10) to which the ware has been subjected, the glaze has of course taken up alumina and silica from the engobe, so that its original composition would not accord, in the *proportion* of these elements, with the formula derived from the analysis of the fired products."

1 Transactions of A. C. S., Volume IV, page 58.
2 Chemistry of Pottery, page 145.

In this case, however, the RO elements and proportions are very different from those in the average white Bristol glaze. Therefore, it seems plausible that the attack of the glaze on the body, is not always governed by high proportions of alumina and silica in the glaze, as stated by Geijsbeek, but also upon the character of the RO fluxes involved.

It will be noted in the experiments to be cited later, that those cone mixtures in which the calcium carbonate is highest require a shorter period of time between starting to bend and going completely down, showing that when the fusion of the mass as a whole once begins, its action is rapid and violent. This same phenomenon is to be seen in brick clays high in lime. In the above glazes just given by Langenbeck, the calcium oxide is relatively very high, which unquestionably causes a rapid fusion similar to that noted in the instances just cited, thus creating a very solvent fluid, which if at all basic, would greedily attack the body, to an extent determined by the length of the burn and the temperature. But whether a Bristol glaze in which sufficient alumina and silica has originally been incorporated "to satisfy the glaze" as we say, and in which the lime is relatively low, will attack the body to any considerable extent is doubtful.

The ratio of alumina to silica to be used in a Bristol glaze is another factor governed by physical as well as chemical limitations. It is a well known fact that the average Bristol glaze can be softened by additions of silica, but the addition of the silica creates physical difficulties that the increase of fusibility cannot counteract, just as in the case of the feldspar before cited.

The writer has made no systematic experiments to determine the effect of various ratios between silica and alumina on various glazes, but has observed their effect in several cases.

Increase in the ratio between silica and alumina is always accompanied by a less opaque glaze, showing that more of zinc has gone into solution.

The writer has not had the privilege of making a microscopic examination of a burned white Bristol glaze, nor has

any record of such an investigation by anyone else been found. But it is probable that the opacity of the glaze, when high in alumina is due, in part, to the formation of crystals. But all the zinc cannot be said to be combined either in the fused portion of the glaze or in these crystals, but is free, either in solution or undissolved. When free, while it would not add to the opacity as much as it would if combined with the alumina, it does add its quotient to the opacity of the glaze. This is shown readily by the addition of more SiO_2 which reacts on the free zinc, taking it into combination with the other silicates.

The physical difficulty is noted in the "dusting" and "scaling" of the glaze high in flint.

The ratio between alumina and silica as found in the best glaze last year[1], *ie*—1 : 5 to 1 : 7, is born out by the following,—

A. S. Watts has given thirty-six porcelain body formulae[2]. He then remarks, "Only in two out of the thirty-six cases does the silica run below 3.7 and in only two cases of modern porcelain does it run over 6.0 SiO_2. As the formulae were figured with alumina as unity, these figures give the direct ratio. Porcelain mixtures are akin to Bristol glazes, the same general laws holding good in each case.

L. E. Barringer[3] found that the salt glaze he examined was essentially the same as could be used for stoneware, having the formula—

$$\left.\begin{array}{l}0.812\ Na_2O\\0.002\ K_2O\\0.182\ CaO\\0.002\ MgO\end{array}\right\}\left.\begin{array}{l}0.612\ Al_2O_3\\0.048\ Fe_2O_3\end{array}\right\}\ 2.704\ SiO_2$$

In this case we find the ratio of alumina to silica to be 1 : 4.41+

Langenbeck[4] gives formulae of glazes analyzed by him as follows:

$$\left.\begin{array}{l}0.26\ K_2O\\0.74\ CaO\end{array}\right\}\ 1.04\ Al_2O_3,\quad 5.76\ SiO_2.\qquad \text{Ratio } 1:5.53$$

1 Transactions of A. C. S., Volume IV, page 72.
2 Ibid, 91, 92, 96.
3 Ibid, 212.
4 Chemistry of Pottery, page 145.

$$\left.\begin{array}{l}{}^{1}0.3544\ K_2O \\ 0.6456\ CaO\end{array}\right\}\ 1.01\ Al_2O_3,\quad 5.53\ SiO_2.\qquad \text{Ratio } 1:5.47$$

$$\left.\begin{array}{l}{}^{1}0.2385\ K_2O \\ 0.3660\ CaO \\ 0.4005\ ZnO\end{array}\right\}\ 0.3307\ Al_2O_3,\quad 1.942\ SiO_2.\qquad \text{Ratio } 1:5.86$$

The same author also gives the analysis and formula of two slip glazes as follows:

$$\left.\begin{array}{l}{}^{1}0.1954\ K_2O \\ 0.4592\ CaO \\ 0.3454 MgO\end{array}\right\}\ \left.\begin{array}{l}0.608\ Al_2O_3 \\ 0.081\ Fe_2O_3\end{array}\right\}\ 3.965\ SiO_2.\qquad \text{Ratio } 1:6.52$$

$$\left.\begin{array}{l}{}^{1}0.229\ KNaO \\ 0.020\ MnO \\ 0.2492\ MgO \\ 0.5015\ CaO\end{array}\right\}\ \left.\begin{array}{l}0.483\ \ Al_2O_3 \\ 0.078\ \ Fe_2O_3\end{array}\right\}\ 3.258\ SiO_2.\qquad \text{Ratio } 1:6.7$$

Geijsbeek in his article "Red Glazes at High Temperature,"[2] quotes several workable alkali—alkaline earth glazes for Cone 8; whose ratio ranged from 4.5 to 5.15.

Seger[3] gives as follows, the formula of three clays which were used as slip glazes:

Glaze from Naumburg RO; 0.40 Al_2O_3; 2.30 SiO_2.
Glaze from Camens RO; 0.68 Al_2O_3; 3.58 SiO_2.
Glaze from Dommitzsch RO; 0.80 Al_2O_3; 3.15 SiO_2.

The ratio of their alumina to their silica are respectively, 5.75, 5.09, 3.937.

A stoneware glaze made by G. Vogt,[4] and used on their stoneware exhibited by the Sevres Porcelain Works, at Paris Exposition, had the chemical formula of

$$\left.\begin{array}{l}0.3\ K_2O \\ 0.7\ CaO\end{array}\right\}\ 0.32\ Al_2O_3,\quad 2.02\ SiO_2.\qquad \text{Ratio } 1:6.31.$$

The ratio of alumina to silica in glazes XI, XII, XIII and XIV before quoted, fall within the limits here indicated.

The following experiments were made to show the relative effect, in various mixtures, of the increase of feldspar at the expense of the carbonate of calcium; and the increase of clay in each case, as shown in the following epitome of the series.

1 Chemistry of Pottery, pages 146-92-84-85.
2 Transactions of A. C. S., Volume I, pages 62-65.
3 Translations of the Collected Writings of Herman Seger, page 452.
4 Volume —— Journal Society Chemical Industry.

Variations in Eqv. of K_2O	Each with a varying ratio of K_2O; Al_2O_3
.200	1: 1.0
.250	1: 1.2
.300	1: 1.3
.350	1: 1.4
.375	1: 1.5
.400	1: 1.6
.450	1: 1.8
.500	1: 2.0

Each of the above has an Al_2O_3: SiO_2 ratio of 1: 5.5 (except in the first numbers of each series, which are 1 : 6.)

Material used: Mayfield ball clay, Brandywine feldspar, ordinary commercial whiting, Goldings flint, and commercial zinc oxide were used in these tests. They were dried thoroughly before being used.

Mode of Manufacture: The first, second and last in each series were ground separately for one hour in a wet ball mill; screened through a 150 mesh sieve; dried to stiffness in plaster molds: and then to dryness over a hot plate and pulverized to pass a 40 mesh sieve.

For the intermediary members, aliquot parts of the second and last members of each series were mixed in dry condition by passing through a sieve several times.

The several mixtures were then made into cones. Each was worked up with water except the first member of each series. In these, sufficient dextrine paste was added (about 10 per cent.) to make the mass workable.

Burning: The burnings were made in an up-draught kiln in which a large muffle was used. The cone slabs were placed far enough to the rear of the muffle so as to prevent those nearest the open end flashing down.

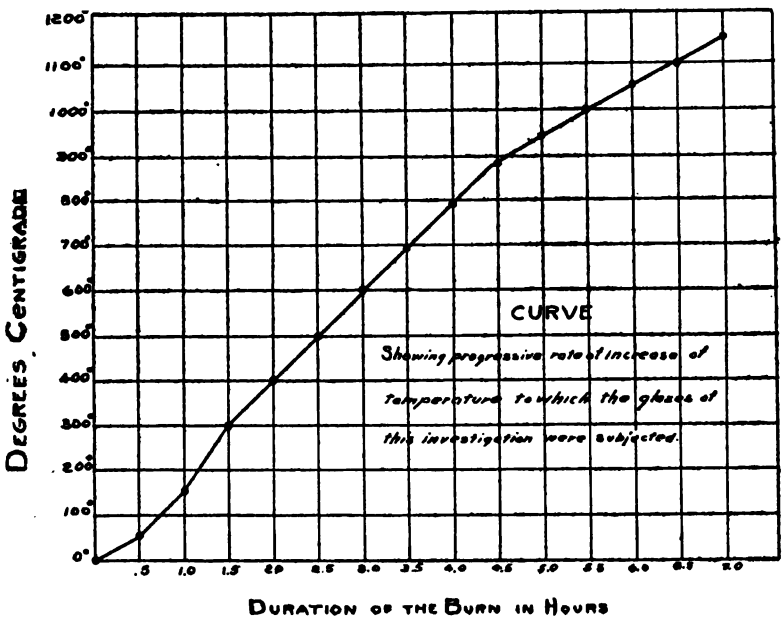
The Le Chatelier pyrometer was used in connection with the Seger cones, to determine the temperature attained.

Series 1, 2, 3 and 4 were burned together on a slab in the first burn; 3, 4, 5 and 6 in the second burn; and 1, 2; 7 and 8 in the third burn, thus checking the pyrometer readings throughout the three burns. This was done because the pyrometer was used between times for several other purposes, the temperature reaching 1300° or more in each case. That the Le Chatelier pyrometer couples will suffer sufficiently by such treatment to differentiate the readings,

was proven by the readings obtained in the last burn. In this burn, the cones in series 1 and 2 came down 15° earlier than they did in the first burn, according to the readings of the pyrometer.

In each burn the following time—temperature curve was followed as closely as possible, after a reading of 120°C was reached, so as to insure similar conditions in each case. (See curve below.)

TRANS. AMER. CERAMIC SOC. VOL. V PURDY-STONEWARE GLAZES



Stoneware Glazes—Series I. RO { 0.20 K_2O, 0.40 CaO, 0.40 ZnO }

Glaze No.	Molecular Formula.			Ratio	Ratio	Fusing Temp. °Cent.	Proportions Blended.
	RO	Al_2O_3	SiO_2	K_2O: Al_2O_3	Al_2O_3 :SiO_2		
I A	1.00	0.20	1.20	1:1.0	1:6.0	1150	I A alone
I B	1.00	0.24	1.32	1:1.2	1:5.5	1150	I B alone
I C	1.00	0.26	1.43	1:1.3	1:5.5	1135	7 parts I B—1 part I H
I D	1.00	0.28	1.54	1:1.4	1:5.5	1100	6 parts I B—2 parts I H
I E	1.00	0.30	1.65	1:1.5	1:5.5	1095	5 parts I B—3 parts I H
I F	1.00	0.32	1.76	1:1.6	1:5.5	1080	4 parts I B—4 parts I H
I G	1.00	0.36	1.98	1:1.8	1:5.5	1075	2 parts I B—6 parts I H
I H	1.00	0.40	2.20	1:2.0	1:5.5	1075	I H alone

Batch Composition of Extremes.

Ingredients.	I A	I B	I H
Feldspar	111.4	111.40	111.4
Whiting	40.0	40.00	40.0
Zinc oxide	32.4	32.40	32.4
Ball clay	——	10.32	51.6
Flint	——	2.40	36.0

Behavior in Fusing.— The cones of this series (I) went down so rapidly when they once started that they required close watching in order to note the time and temperature when they were fused flat, those higher in clay being the most rapid, due no doubt to the action of the CaO on the clay.

Appearance When Cooled. — The last four or IE, IF, IG and IH fused to clear glasses, while the first four IA, IB, IC and ID were semi-opaque, which is contrary to the rule that additions of Al_2O_3 to a glaze increase the opacity of that glaze.

Cone I A had a dead spongy appearance, which was less pronounced, in the others. as they increased in clay.

In this series each addition of ball clay made the mixture more fusible, the point of maximum fusibility not being reached.

Stoneware Glazes—Series II. RO { 0.25 K_2O, 0.35 CaO, 0.40 ZnO }

Glaze No.	Molecular Formula.			Ratio	Ratio	Fusing Temp. °Cent.	Proportions Blended.
	RO	Al_2O_3	SiO_2	K_2O: Al_2O_3	Al_2O_3 :SiO_2		
II A	1.00	0.250	1.50	1:1.0	1:6.0	1125	II A alone
II B	1.00	0.300	1.65	1:1.2	1:5.5	1150	II B alone
II C	1.00	0.325	1.78	1:1.3	1:5.5	1140	7 parts B and 1 part H
II D	1.00	0.350	1.92	1:1.4	1:5.5	1130	6 parts B and 2 parts H
II E	1.00	0.375	2.06	1:1.5	1:5.5	1130	5 parts B and 3 parts H
II F	1.00	0.400	2.20	1:1.6	1:5.5	1120	4 parts B and 4 parts H
II G	1.00	0.450	2.47	1:1.8	1:5.5	1115	2 parts B and 6 parts H
II H	1.00	0.500	2.75	1:2.0	1:5.5	1120	II H alone

Batch Composition of Extremes.

Ingredients.	II A	II B	II H
Feldspar	139.25	139.25	139.25
Whiting	35.00	35.00	35.00
Zinc oxide	32.40	32.40	32.40
Ball clay	——	12.90	64.50
Flint	——	3.00	45.00

Behavior in Fusing.—The cones went down rapidly after they once started.

Appearance When Cooled.—The cones showed their progressive fusibility from II H up to II B by their shape after being cooled. From II H to II D the melted cones were shapeless globules while II D showed semblance to cone shape, II C had round edges, and II B retained its shape and edges.

Stoneware Glazes—Series III. RO { 0.30 K_2O, 0.30 CaO, 0.40 ZnO }

Glaze No.	Molecular Formula.			Ratio	Ratio	Fusing Temp. °Cent.	Proportions Blended.
	RO	Al_2O_3	SiO_2	K_2O: Al_2O_3	Al_2O_3 :SiO_2		
III A	1.00	0.30	1.80	1:1.0	1:6.0	1100	III A alone
III B	1.00	0.36	1.98	1:1.2	1:5.5	1085	III B alone
III C	1.00	0.39	2.14	1:1.3	1:5.5	1075	5 parts B and 1 part H
III D	1.00	0.42	2.31	1:1.4	1:5.5	1045	4 parts B and 2 parts H
III E	1.00	0.45	2.47	1:1.5	1:5.5	1055	3 parts B and 3 parts H
III F	1.00	0.48	2.64	1:1.6	1:5.5	1060	2 parts B and 4 parts H
III G	1.00	0.51	2.80	1:1.7	1:5.5	1075	1 part B and 5 parts H
III H	1.00	0.54	2.97	1:1.8	1:5.5	1095	III H alone

Batch Composition of Extremes.

Ingredients.	III A	III B	III H
Feldspar	167.1	167.10	167.10
Whiting	30.0	30.00	30.00
Zinc oxide	32.4	32.40	32.40
Ball clay	——	15.48	61.92
Flint	——	3.60	41.40

Behavior in Fusing.—These cones behaved more normally, five minutes elapsing between the time they started and the time they were flat.

Appearance When Cooled.—They were all opaque white masses which showed nicely their order of fusibility; cone III D and III E being globules, while the others retained their shape more or less.

It is seen that in this series the point of maximum fusibility is reached when it contains .12 equivalent of clay, .15 equivalent, making the mixture but a trifle harder.

It was not expected that many of this series would fuse down before Seger cone 2 and as the plaque was rather crowded, only so many test cones were used as was judged necessary. So in comparison with Seger cones there was not as much data obtained as was desired. Cone 2, however, did not lose its sharp edges, although lying perfectly flat.

In this quick fire, cone III D or III E would make a good Bristol glaze, but not in the slow stoneware fires.

Stoneware Glazes—Series IV.

RO { 0.35 K_2O, 0.25 CaO, 0.40 ZnO }

Glaze No.	Molecular Formula.			Ratio	Ratio	Fusing Temp. °Cent.	Proportions Blended.
	RO	Al_2O_3	SiO_2	K_2O: Al_2O_3	Al_2O_3: SiO_2		
IV A	1.00	0.350	2.100	1:1.0	1:6.0	1075	IV A alone
IV B	1.00	0.420	2.310	1:1.2	1:5.5	1080	IV B alone
IV C	1.00	0.455	2.500	1:1.3	1:5.5	1045	3 parts B and 1 part F
IV D	1.00	0.490	2.695	1:1.4	1:5.5	1055	2 parts B and 2 parts F
IV E	1.00	0.525	2.887	1:1.5	1:5.5	1065	1 part B and 3 parts F
IV F	1.00	0.560	3.080	1:1.6	1:5.5	1090	IV F alone

Batch Composition of Extremes.

Ingredients.	IV A	IV B	IV F
Feldspar	194.95	194.95	194.95
Whiting	25.00	25.00	25.00
Zinc oxide	32.40	32.40	32.40
Ball clay	——	18.00	54.18
Flint	——	4.20	33.60

Behavior in Fusing:—The cones of this series were still more regular in their time of starting and melting flat, showing the effect of the decrease of carbonate of calcium.

While the most fusible of this series was flat on the plaque at the same temperature reading as III D, it had started five minutes earlier. At this part of the burn, the heat lagged a trifle below the time-temperature curve of the previous burn, thus allowing the cones to complete their coming down a trifle slower than they otherwise would. Their starting, however, was from five to ten minutes earlier than were those of series III, showing that IV C was really a trifle softer than III D.

Appearance when Cooled:—Cones IV B and D were globules while the others retained their shape more or less according to their fusibility. IV A and F appear to be equally fused, but IV F has a gloss, while IV A has only an incipient gloss.

In this series 0.105 equivalent of ball clay can be added before reaching its point of maximum fusibility.

Stoneware Glazes—Series V.

$$RO \begin{cases} 0.375\ K_2O \\ 0.225\ CaO \\ 0.400\ ZnO \end{cases}$$

Glaze No.	Molecular Formula.			Ratio	Ratio	Fusing Temp. °Cent.	Proportions Blended.
	RO	Al_2O_3	SiO_2	K_2O: Al_2O_3	Al_2O_3 :SiO_2		
V A	1.00	0.375	2.250	1:1.0	1:6.0	1070	V A alone
V B	1.00	0.450	2.475	1:1.2	1:5.5	1045	V B alone
V C	1.00	0.487	2.681	1:1.3	1:5.5	1045	3 parts B and 1 part F
V D	1.00	0.525	2.887	1:1 4	1:5.5	1055	2 parts B and 2 parts F
V E	1.00	0.562	3.094	1:1.5	1:5.5	1065	1 part B and 3 parts F
V F	1.00	0.600	3.300	1:1.6	1:5.5	1090	V F alone

Batch Composition of Extremes.

Ingredients.	V A	V B	V F
Feldspar	208.87	208.87	208.87
Whiting	22.50	22.50	22.50
Zinc oxide	32.40	32.40	32.40
Ball clay	—	19.35	58.05
Flint	—	4.50	36.00

Behavior in Fusing:—The cones in this series were five minutes longer from the time they started to the time they were lying flat on plaque.

Appearance when Fused:—They resemble series IV very much, except that they show plainly that the point of maximum fusibility is approaching closer to the lower potash-alumina ratio, V B being considerably softer than IV B.

Stoneware Glazes—Series VI.

$$RO \begin{cases} 0.40\ K_2O \\ 0.20\ CaO \\ 0.40\ ZnO \end{cases}$$

Glaze No.	Molecular Formula.			Ratio	Ratio	Fusing Temp. °Cent.	Proportions Blended.
	RO	Al_2O_3	SiO_2	K_2O: Al_2O_3	Al_2O_3 :SiO_2		
VI A	1.00	0.40	2.40	1:1.0	1:6.0	1065	VI A alone
VI B	1.00	0.48	2.64	1:1.2	1:5.5	1085	VI B alone
VI C	1.00	0.52	2.86	1:1.3	1:5.5	1045	3 parts B and 1 part F
VI D	1.00	0.56	3.08	1:1.4	1:5.5	1060	2 parts B and 2 parts F
VI E	1.00	0.60	3.30	1:1.5	1:5.5	1070	1 part B and 3 parts F
VI F	1.00	0.64	3.52	1:1.6	1:5.5	1095	VI F alone

Batch Composition of Extremes.

Ingredients.	VI A	VI B	VI F
Feldspar	222.8	222.80	222.80
Whiting	20.0	20.00	20.00
Zinc oxide	32.4	32.40	32.40
Ball clay	—	20.64	61.92
Flint	—	4.80	38.40

Behavior in Fusing:—Was similar to series V.

Appearance after Cooling:—While VI F seemed to be fused almost as much as IV F and V F, the other members of the series were more fused than the corresponding members of the other series, showing that this series was more fusible than V.

Stoneware Glazes—Series VII. RO { 0.45 K_2O, 0.15 CaO, 0.40 ZnO }

Glaze No.	Molecular Formula.			Ratio	Ratio	Fusing Temp. °Cent.	Proportions Blended.
	RO	Al_2O_3	SiO_2	K_2O : Al_2O_3	Al_2O_3 : SiO_2		
VII A	1.00	0.450	2.70	1 : 1.0	1 : 6.0	1060	VII A alone
VII B	1.00	0.540	2.97	1 : 1.2	1 : 5.5	1050	VII B alone
VII C	1.00	0.585	3.22	1 : 1.3	1 : 5.5	1065	3 parts B and 1 part F
VII D	1.00	0.630	3.46	1 : 1.4	1 : 5.5	1080	2 parts B and 2 parts F
VII E	1.00	0.675	3.71	1 : 1.5	1 : 5.5	1085	1 part B and 3 parts F
VII F	1.00	0.720	3.96	1 : 1.6	1 : 5.5	1095	VII F alone

Batch Composition of Extremes.

Ingredients.	VII A	VII B	VII F
Feldspar	250.65	250.65	250.65
Whiting	15.00	15.00	15.00
Zinc oxide	32.40	32.40	32.40
Ball clay	——	23.22	69.66
Flint	——	5.40	43.20

Behavior in Fusing.—Very regular.

Appearance After Cooling.—This series, it will be noted, is quite a great deal harder than VI.

The gloss is good. The opacity or whiteness increases in depth with increase of alumina, but this increase in whiteness is accompanied with the same physical defects as was noted in glaze I, before given, when burned on the ware. Pin holes increase in number and size as the alumina increases. In fact, incipient crawling or drawing up into beads is very much in evidence in this series.

Stoneware Glazes—Series VIII. RO { 0.50 K_2O, 0.10 CaO, 0.40 ZnO }

Glaze No.	Molecular Formula.			Ratio	Ratio	Fusing Temp. °Cent.	Proportions Blended.
	RO	Al_2O_3	SiO_2	K_2O: Al_2O_3	Al_2O_3 :SiO_2		
VIII A	1.00	0.50	3.000	1:1.0	1:6.0	1050	VIII A alone
VIII B	1.00	0.60	3.300	1:1.2	1:5.5	1060	VIII B alone
VIII C	1.00	0.65	3.575	1:1.3	1:5.5	1075	3 parts B and 1 part F
VIII D	1.00	0.70	3.850	1:1.4	1:5.5	1080	2 parts B and 2 parts F
VIII E	1.00	0.75	4.125	1:1.5	1:5.5	1095	1 part B and 3 parts F
VIII F	1.00	0.80	4.400	1:1.6	1:5.5	1120	VIII F alone

Batch Composition of Extremes.

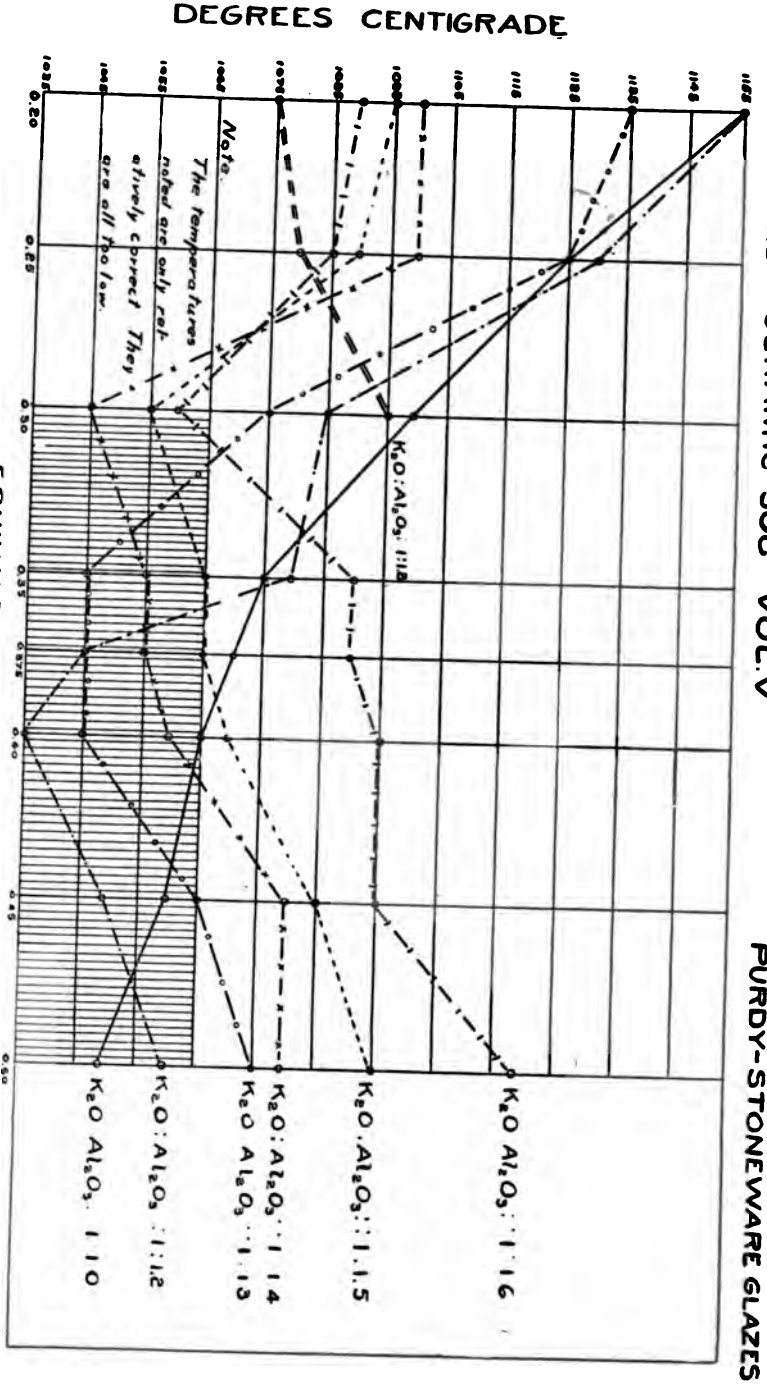
Ingredients.	VIII A	VIII B	VIII F
Feldspar	278.5	275.50	278.50
Whiting	10.0	10.00	10.00
Zinc oxide	32.4	32.40	32.40
Ball clay	——	25.80	77.40
Flint	——	6.00	48.00

Behavior in Fusing.—Very regular.

Appearance After Cooling.—All of this series except VIII A were very much pin holed. This pin hole phenomenon is not due to their refractoriness for they are fused to globules, but rather to their high content of feldspar and low content of whiting.

Curves have been drawn, to study these series as a whole, for the facts are thus more plainly shown. (See page 158.)

From these curves the formula of any of the mixtures can be readily figured. The zinc is retained at 0.4 Eqv. throughout; the potash is given on the abscissa; the calcium-oxide is then determined by difference, making (Eqv. K_2O + Eqv. ZnO + Eqv. CaO) = 1.0; the alumina in any mixture can be obtained by subsituting the equivalent of potash in the ratios which the curve represents, and upon which the mixture is described, and computing for the value of alumina; the silica in all cases is 5.5 times the alumina.


TRANS AMER CERAMIC SOC VOL.V
PURDY-STONEWARE GLAZES
DEGREES CENTIGRADE
EQUIVALENTS OF K_2O
$K_2O:Al_2O_3$ 1:1.8
$K_2O\ Al_2O_3$ 1:1.6
$K_2O\ Al_2O_3$ 1:1.5
$K_2O:Al_2O_3$ 1:1.4
$K_2O\ Al_2O_3$ 1:1.3
$K_2O:Al_2O_3$ 1:1.2
$K_2O\ Al_2O_3$ 1:1.0
Note.
The temperatures noted are only relatively correct. They are all too low.
0.20
0.25
0.30
0.35
0.40
0.45
0.50

The curves show:—

1st. That the most fusible glazes can be obtained with 0.30 to 0.4 equivalent of feldspar, 0.4 making the most fusible one.

2nd. That an increase of feldspar over 0.4 equivalents makes the glaze containing clay more refractory.

3rd. The series in which potash and alumina are as 1 : 1 shows that in each mixture without clay, increase of feldspar increases the fusibility, but as is shown in the following table, the lower the equivalent of feldspar, the larger can the potash-alumina ratio be before the fusing point reaches that of the glaze without clay.

TABLE XII

Showing that a high-feldspar glaze devoid of clay substance, melts at a higher temperature than one containing clay substance, and showing that as the amount of feldspar is decreased, the amount of clay substance must be steadily increased to maintain an equal fusing point.

Equiv. Feldspar.	Equiv. Al_2O_3 at which max. fusibility was attained.	Equiv. clay added for maximum fusibility.	Equiv. Al_2O_3 at which equal fusibility was maintained.	Equiv. clay added for equal fusibility.
0.500	0.50	0.00	0.50	0.00
0.450	0.54	0.09	0.54	0.09
0.400	0.48	0.08	0.56	0.16
0.375	0.4875 0.450	0.1125 0.075	0.5625	0.1875
0.350	0.405	0.105	0.515	0.165
0.300	0.420	0.120	0.540	0.240
0.200	Not reached	Not reached	Not reached	Not reached

4th. The glaze having 0.8 Eqv. of clay added with 0.2 Eqv. of feldspar, or K_2O, is as fusible as the glaze of 0.5 Eqv. of feldspar having only 0.3 of clay added. This is due, no doubt, to the action of the CaO on the clay. The CaO in the first instance amounts to 0.4 and only 0.1 in the latter.

5th. Figuring the equivalent of alumina in the glaze enclosed in the shaded rectangle, representing the most fusible glazes we have, in order of their fusibility for each equivalent of potash:—

TABLE XIII.

No.	K_2O	Al_2O_3	Equiv. of Clay	Temp. of Fusing
1	0.3	0.42	0.12	1045
2	0.3	0.45	0.15	1055
3	0.3	0.48	0.18	1065
4	0.35	0.455	0.105	1045
5	0.35	0.490	0.140	1055
6	0.35	0.525	0.175	1065
7	0.375	{0.450	0.075 }	1045
8		0.4875}	0.1125}	
9	0.375	0.525	0.150	1055
10	0.375	0.5625	0.1875	1065
11	0.40	0.48	0.08	1035
12	0.40	0.52	0.12	1045
13	0.40	0.56	0.16	1060
14	0.45	0.54	0.09	1050
15	0.45	0.58	0.13	1065
16	0.50	0.60	0.10	1060

While these glazes thus tabulated fused considerably under Cone 2, and would give good glossy glazes at Cone 2 or lower, when burned in as short a period of time as this test was, they would require Cone 5-7 in the longer burns of the regular stoneware operations.

In order to verify the results attained in the foregoing investigation, the above sixteen formulae plus 2 per cent. "soluble salt" were given to stoneware potters to be tried as glazes under working conditions. Their percentage compositions are:

TABLE XIV.

Series No.	Distributing No.	Feldspar	Whiting	Zinc	Clay	Flint	"Soluble Salt"
III D	1	59.22	10.68	11.48	10.97	5.74	1.95
III E	2	56.86	10.12	10.93	13.05	7.59	1.96
III F	3	53.77	9.65	10.43	14.94	9.27	1.93
IV C	4	65.72	8.43	10.92	9.13	3.84	1.96
IV D	5	63.18	8.10	10.50	11.71	4.57	1.94
IV E	6	59.04	7.57	9.81	13.77	7.94	1.98
V B	7	71.00	7.70	11.25	6.60	1.54	1.98
V C	8	67.00	7.22	10.40	9.33	3.97	2.00
V D	9	63.25	6.85	9.87	11.80	6.17	1.98
V E	10	60.17	6.50	9.30	13.90	8.10	1.98
VI B	11	72.50	6.53	10.60	6.70	1.60	1.98
VI C	12	68.25	6.18	10.00	9.50	4.05	1.98
VI D	13	64.60	5.80	9.40	12.00	6.28	1.98
VII B	14	75.23	4.50	9.75	6.97	1.62	1.98
VII C	15	70.80	4.25	9.20	9.50	4.37	1.98
VIII B	16	77.50	2.77	9.00	7.20	1.67	1.95

By "soluble salt" was meant what ever the manufacturer was using to prevent crawling.

The Macomb Pottery Company, of Macomb, Illinois, Mr. Charles Kettron, manager, were not running at the time he received the formulae, but as they had a kiln of ware yet to set and burn, they hastily mixed the glazes up by hand, and screened them through a 100 mesh screen. Although imperfectly mixed, they showed very clearly their general behavior.

The heat, as shown by Seger standard cones as made by Prof. Orton, was Cone 7 about half down, Cone 6 having gone down but not flattened. Only in one place of the kiln was Cone 8 "tipped".

The gloss of all the trials was as good as could be expected from hand mixing.

The glazes which contained 0.52 equivalent Al_2O_3 or more, were the most opaque.

The trials were burned in the open, and on the inside of other ware; those burned in the open were a trifle more transparent; otherwise no material difference due to placing

could be noted. Sal-soda was used as the "soluble salt" to prevent crawling. English ball clay was used in all the trials.

Fortunately the trial pieces were half gallon pans which showed more clearly the effect of too high feldspar content than any other piece of stoneware could. Crocks, etc., are bunged, *i e*, placed one upon another allowing considerable space between surfaces, while the pans are nested, *i e*, placed one over the other, the one above resting upon the shoulder of the one below, thus making a comparatively close fit, and leaving but little space between surfaces. It is where the pans fit so closely that the glaze blisters or peels the most readily, that is, around the top edge of the inside and around the shoulder on the outside.

It has been the writers experience that glazes containing over 0.40 equivalent of feldspar, will thus blister when a glaze containing 0.40 equivalent of feldspar, or less, will not. The higher the content of the feldspar the more certainly will this blistering on the pans occur, and 0.50 equivalent of feldspar will invariably cause such blistering.

Another defect due to too high content of feldspar noted in these trials, is the crawling and beading up. Glaze 16 (VII B) was not only very badly crawled but also beaded up, and the inside edges of the pans were badly blistered. Glazes 14 and 15 (VII B and VII C) while not so badly crawled and beaded, were blistered around the inside edges of the pans. The other glazes, however. were practically free from such defects.

This crawling and beading up in glazes, high in feldspar and low in whiting has been noted in every case brought to the writer's attention, both in practice and in the experimental work by classes in the ceramic department, of the Ohio State University. It was even exhibited in the cones made of series VIII, as before noted.

Mr. George W. Shoemaker, chemist for the Robinson-Merrill Pottery Co. of Akron, O., ground the glazes for three hours in small ball mills; dipped them on bone dry ware, the glazes weighing between 24 oz. to 25 oz per pint; placed

them in open saggers in the lower and upper parts of the kiln (up draft, double deck.)

He used borax as the soluble salt; the clay used was china clay. They use Kentucky ball clay in their regular glaze, but Mr. Shoemaker claims to be having better success with this particular china clay.

The temperature attained, as shown by the cones, was very uniform throughout the kiln, cone six just touching the plaque in both top and bottom of the kiln, cone seven being but very slightly bent at the top and perfectly erect at the bottom. They fire the lower part of the kiln by coal in 58 to 60 hours, finishing the upper deck during the latter 10 hours of the burn by the aid of gas.

The trials thus burned in the gas fires of the upper deck were very much whiter than those burned in the coal fires, although the variations in gloss of the various glazes were the same in both parts of the kiln, showing the advantage of burning this type of glaze in purely oxidizing atmosphere.

None of the trials exhibited the crawling to the degree that those made up at Macomb did, due undoubtedly, to the thorough grinding they received.

Taken as a whole, all the glazes as prepared by Shoemaker, had good gloss, and opaqueness. The poorest one was as opaque as any to be seen on the market.

No. 16 or VIII	B	Very white, good gloss.
No. 15 or VII	C	Very white, good gloss.
No. 10 or V	E	But a shade darker than 15.
No. 18 or VI	D	But a shade darker than 15.
No. 14 or VII	B	But a shade darker than 15.
No. 3 or III	F	A trifle darker shade than the previous group, good gloss—considered fine white bristol glazes.
No. 4 or IV	C	A trifle darker shade than the previous group, good gloss—considered fine white bristol glazes.
No. 5 or IV	D	A trifle darker shade than the previous group, good gloss—considered fine white bristol glazes.
No. 6 or IV	E	A trifle darker shade than the previous group, good gloss—considered fine white bristol glazes.
No. 8 or V	C	A trifle darker shade than the previous group, good gloss—considered fine white bristol glazes.
No. 9 or V	D	A trifle darker shade than the previous group, good gloss—considered fine white bristol glazes.
No. 11 or VI	B	A trifle darker shade than the previous group, good gloss—considered fine white bristol glazes.
No. 12 or VI	C	A trifle darker shade than the previous group, good gloss—considered fine white bristol glazes.
No. 1 or III	D	Still darker, but would be considered very good white bristol glazes.
No. 2 or III	E	Still darker, but would be considered very good white bristol glazes.
No. 7 or V	B	Still darker, but would be considered very good white bristol glazes.

The darkening of the shade, above referred to, is the decreasing of opaqueness, the yellow body showing through more plainly in the last group.

By comparing the percentage composition of the glazes the following facts are to be noted:

1st. The percentage amount of zinc used in the first group or 16, 15, 10, 13, 14, were 9.0, 9.2, 9.3, 9.4, 9.75 per cent. respectively as against 11.48, 10.93, 11.25 per cent. of the last and opaque group.

2nd. That this increase of opaqueness, notwithstanding the decrease in zinc, is due to the alumina is obvious. Comparing the average equivalents of alumina in the three groups, we have for the most opaque or group No. 1, 0.5685 Al_2O_3; for next groups, 0.4953 Al_2O_3; and for the next or least opaque 0.44 Al_2O_3. But is this increase of opaqueness between these three groups in proportion to the increase of alumina, and is it due entirely to this increase of alumina?

Comparing the average ratio of carbonate of calcium to clay in the three groups, we find for the first group 2.11; the second group, 1.43, and the third group, 1.04, *i. e.* for one part of whiting in the first group we have 2.11 parts of clay, etc. Thus, while the carbonate of calcium is decreasing in equivalents as well as in percentage amounts, there is an increase in the ratio of calcium carbonate to clay as well as an actual increase in content of feldspar.

A study of the fusion curve in the light of the experiment of Bleininger before quoted, shows the action of the calcium oxide in the several amounts. In series I with 0.4 $CaCO_3$ the maximum fusibility was not reached even with 0.16 equivalent of clay, and the fusibility of the mixtures decreased with the decrease of clay. Through the other series, this condition was being reversed regularly with decrease of $CaCO_3$ until in series VIII every addition of clay made the glaze more refractory.

In the case of glaze VIII B (16) the 0.1 equivalent CaO, has "spent" itself upon the clay leaving, possibly, some clay undecomposed to play its role as a refractory ingredient, the feldspar being the principal solvent medium.

As has been pointed out, mixtures in series VIII were very steady and slow in fusing, indicating a less active fluxing condition. Therefore more zinc would remain uncombined, and the glaze composition would be that of the slightly

opaque potash-alumina-silicate instead of the clear lime-zinc-alumina-silicate as in the case where sufficient CaO is present to decompose all the clay.

The answer then to the query of why, as the feldspar increases, less clay can be added before raising the fusibility of the mixture is, that with the increase of feldspar there is a decrease of $CaCO_3$ the active agent in the decomposition of the clay and feldspar, which renders them in a condition for more ready fusion.

The effect of too high content of feldspar in the Akron trials, was not so much in evidence as in the trials from Macomb, due probably to the more thorough grinding. Yet, 16 (VIII B) was crawled and had incipient blisters.

Of the various mixtures tried 13, (VI D) is the most successful and economical glaze, and its formula 0.4 K_2O, 0.2 CaO, 0.4 ZnO, 0.56 Al_2O_3, 3.08 SiO_2 would be the one recommended for stoneware at Cones 6-7. For higher heats the Al_2O_3 and SiO_2 can be increased, always keeping their ratio 1 : 5.5; for lower heats the Al_2O_3 and SiO_2 can be decreased, but never below 0.48 Al_2O_3, always keeping the ratio of Al_2O_3 SiO_2, 1 : 5.5.

DISCUSSION.

Mr. Stanley G. Burt: I would like to ask, in this series of Bristol glazes, whether the lower-fusing formulae retain their opaqueness?

Mr. Purdy: I expect they will. The opaqueness, as I stated last year, is due, in part, to alumina, and it was questioned whether so much alumina as 0.5 could be retained, and not affect materially the fusibility of the glaze. In my paper last year, there were several formulae given in which zinc was higher than four-tenths. These glazes when made up and fired, crawled, and when made lower than that in zinc they were not white. I, therefere, chose four-tenths zinc. Four-tenths zinc, however, will not cause opacity when the alumina is less than 0.40, and further addition of alumina increases the opacity. Therefore, I believe we are safe in assuming that these lower-fusing formulae will retain their opaqueness.

Mr. Burt: Your white means opacity?

Mr. Purdy: Yes, sir.

Mr. Burt: I think it would be interesting to have one not showing opacity. It is a question whether we haven't there the making of a clear leadless glaze.

Mr. Purdy: An addition of five hundredths of lime at the expense of the zinc will clear the glaze up.

The Chair: Did you ever try to replace the zinc with oxide of tin for whiteness?

Mr. Purdy: Yes, sir. I would like to have some one explain to me why, with four-tenths of an equivalent of feldspar, we have the most fusible glaze, and why any addition of feldspar above that point makes it more refractory? With this four-tenths equivalent of feldspar, we have also two-tenths lime, which, with the four-tenths zinc makes up the RO.

The Chair: What is the amount of the lime in those glazes where you increase the feldspar and which become more refractory?

Mr. Purdy: In these trials, lime was not added in sufficient quantity to have refractoriness due to the excessive lime. But we do have refractoriness when the spar is increased *at the expense of* the lime.

The Chair: It may be a little like adding lime as a flux in a vitreous body. It helps to bring the body to vitrification up to a certain point, but an increase of lime above that, goes the other way again.

Mr. Langenbeck: I think Mr. Purdy's paper is extremely interesting on account of the side lights it has thrown upon the behavior of glazes under different conditions of firing in general; particularly his observation of the maturing of some of the glazes of this series at a lower temperature under rapid fire, than in a regular kiln in which there is a large mass of ware. A great many must undoubtedly have been puzzled as I have, by the apparently radical difference in behavior of glaze trials in a small experimental furnace and in a large kiln. And I think, furthermore, that his observations on the conditions under which clay and lime are brought into chemical activity are of far-reaching import,

not only in glazes, but in fluxing bodies for vitrification; it seems to me, it would be an interesting starting point for many different lines of experiment. I know in a general way, of course, that the relations of feldspar to alumina and silica, with reference to vitrification and after fusion, vary very strangely according to proportions. We know this in a general way from Seger's experiments along this line; but I don't know at all, even with the amount of work done, and the contributions which Mr. Purdy has added, that any theory yet can be developed—I don't know of anyone who can answer Mr. Purdy's question to explain why the increase of feldspar beyond four-tenths equivalents causes the body to become more difficult to fuse. Undoubtedly there is a law, and when we get the many facts together; we can develop the law which will be a guide to future work. The idea that an increase of basic elements necessarily increases fusibility at the same time, was long ago upset.

If you take the case of vitreous porcelain bodies, it is frequently desirable to increase the fusibility of the body, and for practical reasons you must have a certain amount of clay present in order to produce a workable, sufficiently plastic mass. But since your feldspar already carries so much alumina with it, you must reach your alumina and silica with some more fusible flux. It doesn't make a particle difference whether the flux is more fusible than feldspar; its combination with clay is much more fusible than you can make it with any feldspar addition. And I would like to say in that connection, that the ordinary method of guaging the fluxing properties of feldspar which the potter uses, viz., putting it in the kiln and seeing how it fuses in comparison with other spars, is incorrect. The assumption that because one flux is more fusible than another *per se*, it follows that the use of the more fusible flux is going to make a body more fusible, is not necessarily the case. Mr. Purdy's experiments demonstrate that.

FURTHER CONTRIBUTIONS TO THE MANUFACTURE OF ARTIFICIAL SANDSTONE OR SAND BRICK.*

BY

S. V. PEPPEL, B. SC., COLUMBUS, O.

This contribution is the result of work carried out to determine the allowable impurities in raw material and the conditions of manufacture which will produce the best sand-brick. I would suggest that the use of the term "sand-brick" be limited to those bodies which consist of a mass of sand particles bound together by a net work of calcium-, calcium-magnesium-, or magnesium-silicate, formed by the action of steam under pressure upon silica or quartz and calcium or magnesium hydrates or a mixture of both of them.

Illustration No. 1, shows the fractured surface of such a body, enlarged eight diameters.

In any industry, and especially in a young one, there are always many questions which are the occasion of a great deal of uncertainty both in selection of raw materials and machinery for manufacture. When an industry has got well started, we know that with certain raw material, handled in a definite manner, in a certain machine or combination of machinery, certain desirable results can be accomplished. We also know that a certain raw material exactly like in every respect to that in use, is not always available, or if available there may be something else of little value for any other purpose, which could be just as well used, and sometimes with better results. And farther, it frequently happens that the first machines in use are not the most economical, or do not produce the best possible product.

It was to settle as many of these questions as possible that the work which I am about to report upon was instituted. We set out with a view of determining the effect of the ordinary impurities in the class of materials which in a comparatively pure state, has been used in the manufacture of sand-brick and to learn what was required of each machine,

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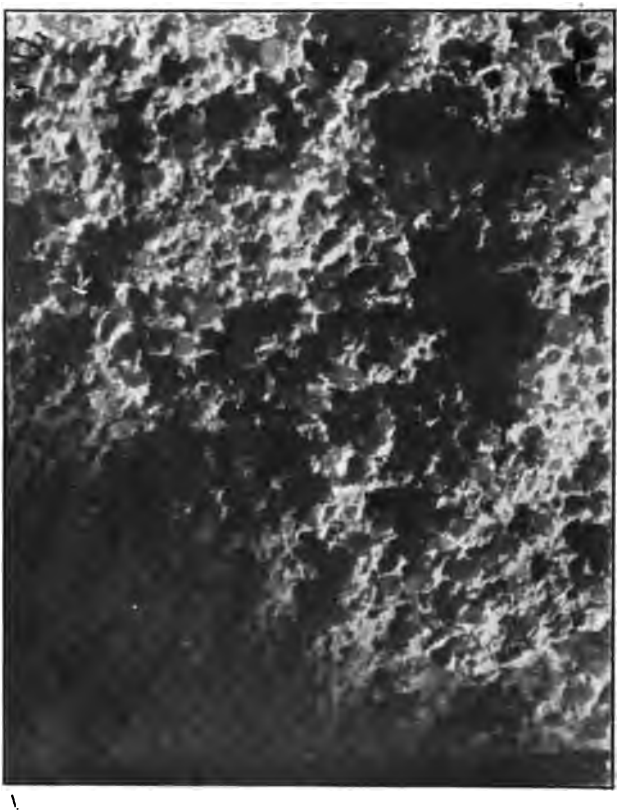


SHOWING THE FRACTURE OF A SAND BRICK
ENLARGED 8 DIAMETERS.

with a view to pointing out their faults, if any could be found.

In order to do this, it was necessary to establish an artificial stone factory, on a small scale, so constructed that we could control the working conditions exactly. The forms into which our product was made were only two, one having the shape of the ordinary cement brickette to be used in determining tensile strengths, and a two inch cube for testing the crushing strength. We had cast-iron molds and dies made for producing these two shapes, expecting to use a hand brick press for the necessary power. For a cylinder in which to harden the brick, we used a piece of ten inch wrought-iron pipe, fitted up with flanges and cast-iron heads on each end. For steam pressure we had access to a 100-horse power boiler, capable of carrying steam up to 250 pounds pressure.

Although we thought this outfit ample and satisfactory for experimental work, we soon found our troubles.

After a few trials, had to abandon the hand press, since we could neither get pressure enough nor control the pressure uniformly. We surmounted this difficulty by placing our dies between the heads of a Tinius Olsen 100,000-pound testing machine, where we could not only give as much pressure as we wanted, but could also control every movement and record exactly what we were doing.

We will take up the discussion of the subject under six heads, although in the nature of the case the working out of the problems under each head interlocked and overlapped each other at many points. The facts fall naturally under the following heads:

1. The raw material and its preparation
2. The behavior of the mixture in the press.
3. Hardening.
4. Testing.
5. Mechanical equipment.
6. Discussions of the merit of the various systems, including patents.

I—THE RAW MATERIAL AND ITS PREPARATION.

As before stated, the sand-bricks consist of a mass of sand particles bound together into a rocklike structure by a network of lime or magnesium silicates. The raw materials consist of only two principal substances, the sand and the quicklime. They will be discussed in turn.

A—THE SAND.

First. Almost any sand *can* be used, and fair product produced, if the treatment is properly varied to suit the physical and chemical properties of the sand. Economy in manufacture, however, limits both physical and chemical properties.

While a good-appearing brick, moderately strong at the time it is taken from the hardening kettle, can be made from almost any kind of sand, still for a safe and durable material of construction, there are certain limitations in the chemical, or rather mineralogical make-up of the sand, aside from those dictated by economy in manufacture, as will be shown later by experimental results. A sand with too much clay in it will make a brick which will not stand up long under the attacks of severe weather.

Second. A comparatively pure sand is essential to cheap manufacture, as well as for the production of a safe and durable material, nor should the sand be too coarse. For the best results it is probable that most of it should pass through a screen, 20 meshes to the inch, unless there is a good gradation from coarse to fine particles, in which case a coarser sand might be used.

Since sand constitutes the major portion of the raw material for this industry, we will take up the examination of it first.

Beginning with comparatively pure sand, the first problem is, what proportions of coarse and fine sand are best. Work done by Prof. Glasenapp showed that some fine sand is necessary. Dr. Michaelis has said that two-thirds coarse sand and one-third fine give the best results. This statement, of course, is a little indefinite, since coarse and fine sand are not defined. Reason tells us that the mixture

which will leave the smallest amount of interstitial space when pressed together, will be the best. This might be calculated exactly. But since such a sand would not be attainable in a commercial way, the calculation is omitted, and practical tests will be given to show the effect of varying amount of very fine sand.

In all the work which follows a pure, sharp glass-sand was used. On giving it a mechanical analysis, it was found to be made up of the following sizes:

20 to 40 mesh—50%
40 to 60 mesh—33%
60 to 80 mesh— 7%
80 to 100 mesh— 7%
100 to 120 mesh— 2%
120 to 150 mesh— 1%

Since this sand showed no very fine grained particles, they were supplied in the form of a pure potters flint. The mechanical analysis of the flint was as follows:

100 to 120 mesh—0.70%	Sieve Separation.
120 to 150 mesh—1.00%	
150 to 200 mesh—1.25%	
.00212 in. diam.—24.00%	Sedimentation Separation.
.00086 in. diam.—19.00%	
.00040 in. diam.— 8.50%	
.00026 in. diam.— 7.50%	
.000186 in. diam.— 5.70%	
Too fine for measurement—32.55%	

Both sands were practically free from soluble silica.

In order to test the influence of the different proportions of the two sands, a series of cubes and brickettes were made up and hardened. All the conditions of manufacture were maintained as nearly uniform as possible. Five per cent. caustic lime and five per cent. of water above that needed for hydration were used. The lime used in these tests was steam slaked. The results here given are the average of three tests of each kind:

TABLE I.

Number	Composition.		Crushing Strength Pounds per sq. inch.	Tensile Strength Pounds per sq. inch.
	Parts Coarse.	Parts Fine.		
77	8	2	3114	131
79	4	2	2955	144
84	3	2	2461	224

These results are very instructive and clearly indicate that much more work might well be done along this line.

It is evident from the above figures that a decrease in the proportion of the coarse sand, decreases crushing strength, and that it increases tensile strength. While this is the case for the range of mixtures examined, I think when carried further, limits will be reached in both directions, and I regret very much that we are unable, at present, to fix these limits.

The blocks for the preceding test were made in the hand brick machine, on which it was impossible to give each block exactly the same pressure as the last. Since, as we shall show later, the pressure in making has a decided influence on the strength, the above results would probably show a close relation between the decrease in crushing strength and the increase in amount of very fine particles, if they had been made in a machine where this pressure was under accurate control.

From the foregoing I would conclude that in order to maintain the best conditions, and using a sand all of which would be retained on a 40 mesh screen, we should have to grind one-fourth of the sand so that at least one-half of it would pass a 150 mesh screen. If, however, we had a sand with a good assortment of sizes from coarse to fine, we ought to have sand finer than 150 mesh at least equal in amount to the weight of lime to be added.

The fine sand not only fills the spaces between the coarser particles, but assists in the even distribution of the lime, and very much accelerates the chemical reaction so necessary to strength.

ORDINARY IMPURITIES.

The ordinary impurities in sands are for the most part silicates, represented by clay, mica, feldspar, etc., and almost always some ferric oxide. Mica and feldspar are frequently absent, but clay and oxide of iron are seldom absent entirely. While in the general run of cases, feldspar and mica are not present in any important quantity, there are regions in which the sands are rich in one or the another, and sometimes

in both. Oxide of iron, which is almost always present in greater or less quantity, reacts with silica very slowly, if at all, in the presence of steam under pressure; it may therefore be considered inert except for its coloring action, unless it acts as a promoter of chemical combination, as is the case with ferric oxide in the manufacture of Portland cement. No attempt was made to determine whether this is the case or not. By far the most wide spread impurity in sand is clay, or kaolinite. If there is any impurity in sand, clay is nearly always there, and there are many sands which carry considerable quantities of clay. Its effect on the sand-brick process was therefore studied in more detail than that of any of the other impurities.

No tests were made with mica, since it would seldom be found in sufficient quantity in sand to extend much of an influence. Prof. Rinni,[1] of Hanover, commenting on the examination of a number of sections under the microscope, says: "Mica flakes do not seem to have been materially attacked. Quartz and feldspar do not show much if any difference."

The Influence of Kaolin, as an Impurity in Sands.

The action of kaolin was examined as representing the hydrous silicates like clay, and feldspar as representing the more complex anhydrous silicates.

The experimental series of mixtures designed to test the influence of clay as an ingredient of sand are set forth in the following tables II to VI. The mixtures were made up into cubes and tensile test pieces in each case, in as uniform a method as possible.

In Tables II and III, five per cent. of the same lime was used in each case, but in Table IV, five per cent. of a different lime was used. In Tables II and IV the ratio of coarse particles to fine particles in the sand mix was 4 to 1; and in Table III, 3 to2. In Table V the coarse to fine was 2 to 1, and ten per cent. of lime was used; in Table VI, 2 to 1, and ten per cent. of dolomite; thus, the tables are not strictly comparable with each other. However, in each table, the only variable is the amount of kaolin which was used to

1 Thon Industrie Zeitung, 1908, No. 16, p. 158.

replace the flint or fine material. Tables V and VI are the averages of a comparatively large number of tests.

TABLE II.

Data:—Molding Pressure—10000 pounds per square inch.
Steam Pressure—150 pounds per square inch.
Temperature in Hardening Cylinder—185°C.
Time Exposed to Steam—10 hours.

Number of Test.	Composition of Mixtures.					When Tested.						Per cent. water absorption.
	Parts Coarse Sand	Parts Fine Sand. (Including impurities.)	Percents Kaolin.	Per Cent. Qu'ckl'me		At once after hardening		After ageing		After freezing		
				Pure CaO	Dolomitic	Crushing Strength	Tensile Strength	Crushing Strength	Tensile Strength	Crushing Strength	Tensile Strength	
85	4	1	2.5	....	5	2766	338	2449	194	2917	219	8.32
86	4	1	5.0	....	5	2500	210	2376	277	2481	181	8.00
87	4	1	10.0	....	5	1943	184	1687	157	1910	121	8.50
88	4	1	20.0	.. .	5	1705	162	1325	138	1477	93	9.00

TABLE III.

83	3	2	....	...	5	3697	427	....	....	3812	215	8.06
89	3	....	2	....	5	2260	238	1846	187	2117	156	10.36

TABLE IV.

93	4	1	2.5	5	....	3835	351	3955	364	4502	352	8.62
94	4	1	5.0	5	....	3340	295	3342	175	3887	269	8 60

TABLE V.

Data:—Molding Pressure—15000 pounds per square inch.
Steam Pressure—150 pounds per square inch.
Temperature in Hardening Cylinder—185°C.
Time Exposed to Steam—10 hours.

Number of Test.	Composition of Mixtures.					When Tested.						Per cent. water absorption.
	Parts Coarse Sand	Parts Fine Sand. (Including impurities.)	Percents Kaolin.	Percent Qu'ck l'me		At once after hardening		After ageing		After freezing		
				Pure CaO	Dolomitic	Crushing Strength	Tensile Strength	Crushing Strength	Tensile Strength	Crushing Strength	Tensile Strength	
A*	2	1	....	10	....	7745	437	.....	.....	9007	371	8.62
C†	2	1	10	10	....	5872	593	.....	.....	6194	561	6.41

*A is the average of tests Number 106, 107, 108, 109, 110 and 111.
†C is the average of tests Number 118, 119 and 120.

TABLE VI.

B*	2	1	....	....	10	5187	286	.....	....	5853	314	9.11
D†	2	1	10	...	10	4429	445	.....	.....	4818	459	8.05

*B is the average of tests Number 112, 113, 114, 115, 116 and 117.
†D is the average of tests Number 121, 122 and 123.

TABLE VII*.

89	3	....	2	....	5°	2260	238	1846	187	2117	156	.....
92	3	...	2	....	20	4729	599	3780	596	5797	480	.....
83	3	2	....	....	5	3697	427	.....	.....	3812	.....	.....
90	3	2	....	....	10	5607	503	5843	446	7525	417	.....

*Conditions of manufacture same as Table III.

Reviewing the facts expressed in tables II, III and IV, where the brick are made under a pressure of 10,000 pounds per square inch, we observe that with each addition of kaolin there is a marked decrease in both tensile and crushing strength, this being most marked in table III, where in test 83 the proportion of flint was large and in test 89, where all of this flint was replaced by kaolin. Also we observe that the decrease in crushing strength is at least one-third with the addition of 20 per cent. of kaolin.

Tables V and VI, the blocks for which were made under

a pressure of 15,000 pounds per square inch, tell a different story. Here again with the addition of kaolin, there is a marked falling off in crushing strength, accompanied by a very decided increase in tensile strength. The effect of the pressure used in molding the block will be discussed under pressing.

Coming back to tables II and III, we observe number 88, 20 per cent. kaolin, shows a very decided loss in strength after the freezing test, as well as on ageing for 35 to 40 days. This is also the case with number 89, which is very rich in kaolin.

The increase in strength shown here as due to freezing, will be discussed in connection with the effect of frost in general on all kinds made.

It was next thought that an increase in lime might overcome the ill effects of a large amount of kaolin, and to test this number 92 was prepared and made under the same conditions as number 89, except that 20 per cent. of lime was added instead of five per cent. This test proved that an increase in lime did in a measure counteract the influence of high kaolin. In comparing figures in table VII, it will be noted that between 89 and 92 there is a marked increase in strength, the tensile strength being more than doubled, and that after the freezing test, number 92 shows an increase in crushing strength when number 89 shows a decrease. However, going farther and comparing numbers 83 and 90 in which there is no kaolin, and in which the lime was only increased from five to ten per cent., we see that there is approximately as much increase in strength for the addition of ten per cent. here, as there was with 20 per cent. in the preceding case where kaolin was a large part of the mixture. Another very pertinent point to be observed is that the increase in strength after freezing is very much greater in the case of numbers 83 and 90 than it is with 89 and 92.

So that while an increase of lime does in a measure overcome the bad effect of clay in large amount, we are safe in saying that any considerable amount of clay in a sand to be used in this industry, is very undesirable, since to use enough lime to make it a safe product, would make it too

expensive, and after all the product would not be the best.

From the preceding facts it would appear that clay up to ten or twelve per cent. is probably not dangerous, and possibly as small an amount as two and a half per cent. might be desirable.

Clay adds to the ease in molding. Owing to its smooth unctuous nature, it acts as a lubricant and decreases the friction on the mold.

The influence of feldspathic minerals as an ingredient of sand.

The effect of feldspar was not examined in the same detailed manner that kaolin was, since it is not so common an ingredient, in quantity. However, a number of blocks composed of sand containing ten per cent. of fine feldspar were made, using a pressure of 15,000 pounds to the square inch. The average result of all the tests on this are given under F in Table VIII. The tests grouped under B are parallel in every way, except that no feldspar was introduced into the sand.

TABLE VIII.

Data:—Molding Pressure, 15000 pounds per square inch.
Steam Pressure, 150 pounds per square inch.
Temperature in Hardening Cylinder, 185°C.
Time Exposed to Steam, 10 hours.

Number of Test.	Composition of Mixtures.					When Tested.						
	Parts Coarse Sand.	Parts Fine Sand (Including Impurities.)	Per Cent. Feldspar.	Per Cent Qu'ckl'me		At once after Hardening.		After Ageing		After Freezing.		Per Cent. Water Absorption.
				Pure CaO.	Dolomitic.	Crushing Strength.	Tensile Strength.	Crushing Strength.	Tensile Strength.	Crushing Strength.	Tensile Strength.	
B*	2	1	0	....	10	5187	286	.....	.....	5853	314	9.11
F†	2	1	10	..	10	4619	339	.....	.....	5115	197	6.94

*B's the average of tests number 112, 113, 114, 115, 116 and 117.
†F's the average of tests number 124, 125 and 126.

Studying Table VIII we note a slight decrease in crushing strength, and an increase in tensile strength, between B and F. After freezing, F increases in crushing strength, but not at the same rate as B. The only erratic factor in

the series is the marked falling off of the tensile strength after freezing.

On the mechanical tests alone we would hardly be justified in excluding a sand which carried as much as ten per cent. feldspar. However, from a few preliminary experiments, I know that both quicklime and caustic magnesia react on feldspar to some extent when exposed to steam at 150 pounds pressure for ten hours, and this reaction can hardly happen without the liberation of potassium or sodium salts and since the solubility of all the salts of these metals is so well known, the danger of effloresence or scumming from this source would be great, if any appreciable amount of the feldspar were attacked by the lime. Until it is definitely proven that feldspar in lime-sand-brick does not produce scumming, I should be cautious about using such a sand, if I desired a first-class product when working at high steam pressure.

If, on the other hand, the feldspar molecule opened up, and took on calcium-oxide and silicic acid, (SiO_2) making a more complex and insoluble silicate, there would be no danger of scumming, and we should have a stronger brick for each unit of calcium-oxide used. The results tend to show that this complex silicate is not built up, since the brick with feldspar were slightly weaker, rather than stronger.

A preliminary experiment made at 150 pounds steam pressure and ten hours time showed quartz to be more attacked by lime than feldspar was. From this we might infer that sands rich in feldspar could safely be worked, by keeping the steam pressure down to 120 pounds or lower, and by not allowing the bricks to remain in the cylinder any longer than necessary to get enough quartz into combination to produce a good bond. Working in this way it is probable that not enough feldpars would be broken up to give any trouble later.

The influence of soft-burnt clay as an ingredient of sand.

On account of the hydraulic properties developed by mixtures of weakly-ignited clays and caustic lime under ordinary atmospheric conditions, it was considered advisable

to test their behavior under high pressure steam. As can be readily seen from Table IX, nothing would be gained by the use of dehydrated kaolin, since the blocks made with fine sand under the same conditions of manufacture developed double the strength shown when dehydrated kaolin is used.

TABLE IX.

Data:—Molding Pressure, 15000 pounds per square inch.
Steam Pressure, 120 pounds per square inch.
Temperature in Hardening Cylinder.—180°C.
Time Exposed to Steam, 8 hours.

Number of Test.	Composition of Mixtures.					Tests.	
	Parts Coarse Sand.	Parts Fine Sand (Including Impurities.)	Parts Burnt Kaolin.	Per Cent. Quicklime.		Crushing Strength	Tensile Strength
				Pure CaO.	Dolomitic.		
A 8	2	1	.....	10	.....	8545	.....
I 8	2	.....	1	10	.....	3919	459
B 8	2	1	.....	.....	10	6038	.....
J 8	2	.....	1	.....	10	3110	256

This concluded the work on the impurities of the sand.

While it was very probable indeed that sharp sands are much to be preferred, I regret that we have no comparative tests to show just how much difference the shape of the grains will affect the strength of the material produced.

B—THE QUICKLIME.

The work of Prof. Glasenapp, cited by us in an article "The Manufacture of Artificial Sandstone", in Volume IV, of this society's transactions, showed the necessity of at least a small amount of very fine sand in order that the chemical reaction may not be too much retarded by lack of intimate contact between sand and lime particles. This brings us to a consideration of the lime.

Quantity Required.

Naturally, the first query is as to quantity. Practice ranges from five to ten per cent. In order to determine the desirability of using large or small amounts of lime, it was

necessary to find out if the strength increased with an increase of lime, and if so, whether the increase was proportional to the increase in lime. To get these facts the following series of test were made, as shown in Table X.

TABLE X.

Data:—Molding Pressure, 10000 pounds per square inch.
Steam Pressure, 150 pounds per square inch.
Temperature in Hardening Cylinder, 185°C.
Time Exposed to Steam, 10 hours.

Number of Test.	Composition of Mixtures				When Tested.						
	Parts Coarse Sand.	Parts Fine Sand (Including Impurities.)	Per Cent. Quicklime.		At once after Hardening.		After Ageing		After Freezing		Per Cent. Water Absorption.
			Pure CaO.	Dolomitic.	Crushing Strength.	Tensile Strength.	Crushing Strength.	Tensile Strength.	Crushing Strength.	Tensile Strength.	
83	3	2	....	5	3697	427	....	....	3812	....	8.06
90	3	2	....	10	5607	503	5843	446	7525	417	9.87

TABLE XI.

84	3	2	5	....	2636	194	....	....	....	....	10.25
97	3	2	40	....	7018	541	7153	622	7995	516	12.13

Studying Tables X and XI, we see in Table X, where dolomite lime was used, an increase in strength with an increase in lime, and also that while the lime was doubled, the increase in strength was only a little over 50 per cent. After freezing, the sample containing the increased lime does show almost 100 per cent increase in strength. However, this increase is probably mainly due to the changing of free lime into carbonate, by the carbonic acid in the water which was used to thaw out the bricks after each freezing.

In Table XI, a high-calcium lime was used, and extremes of high and low lime were compared, using eight times as much in number 97 as was used in number 84. With an eight-fold increase of lime, only a three-fold increase of strength was produced

We might then conclude from the foregoing, that while there is an increase in strength with an increase in lime, this

increase is less marked the higher the lime becomes, and that the strength gained by the addition of lime beyond ten per cent. would not justify the additional expense. Therefore, we may say that present practice in this respect is very good.

It should be stated here that the percents of lime given in this paper are all based on the raw sand as unity. That is, five per cent. lime means that five pounds of lime was added to 100 pounds of sand.

Pure vs. Dolomitic Limes.

Having decided how much lime we should use, the next question which presents itself is whether some particular type of lime will serve our purpose better than any other? Will a hydraulic lime be as good or better than a pure lime? Will lime made from dolomite be as good as one made from a high-calcium limestone?

In view of the facts shown in Table IX, that lime and dehydrated kaolin do not develop any strength under high pressure steam, we would infer that steam under pressure is not conducive to the development of the hydraulic properties of a hydraulic lime. We would therefore expect that the calcium-silicate of such a lime would be inert so far as developing immediate strength in a sand-brick is concerned. This may be due to the dearth of moisture, since hydraulic lime is best in wet places. The hydraulic properties of a lime might develop slowly in the brick after manufacture, and at the end of a year show great strength, especially if the brick were in contact with water. This is, however, an open question as yet.

We naturally consider pure limes the best, if carefully burned, since for each unit of weight we have available a greater proportion of the active substance, namely, the oxides of calcium and magnesium.

Aside from the preceding considerations, we have in most States, two distinct types of limes. In some localities both are available at approximately the same cost; in others there would be a marked difference in price. These limes are known as a *gray* lime, made from a limestone carrying from 85 to 100 per cent. carbonate of calcium, usually with but

little carbonate of magnesium present. The other is known as *white* lime, and is made from dolomite or dolomitic limestones and usually carries about 42 to 44 per cent. carbonate of magnesia and 56 to 58 per cent. carbonate of calcium; that is to say, a molecule of each carbonate go into chemical union with each other.

To test the relative value for the sand-brick reaction of these well marked commercial varieties of lime, the following tests were undertaken:

TABLE XII.

Data:—Molding Pressure, 15000 pounds per square inch.
Steam Pressure, 150 pounds per square inch.
Temperature in Hardening Cylinder, 185°C.
Time Exposed to Steam, 4 to 14 hours.

Number of Test.	Composition of Mixtures.				When Tested.				
			Per Cent. Quicklime.		At once after Hardening.		After Freezing.		
	Parts Coarse Sand.	Parts Fine Sand (Including Impurities.)	Parts CaO.	Dolomitic	Crushing Strength.	Tensile Strength.	Crushing Strength.	Tensile Strength.	Per Cent. Water Absorption.
A*	2	1	10	....	7745	437	9007	371	8.62
B†	2	1	....	10	5187	286	5853	314	9.11

*A is the average of tests number 106, 107, 108, 109, 110 and 111.
†B is the average of tests number 112, 113 114, 115, 116 and 117.

Twelve blocks were tested in A, and an equal number in B, all made in exactly the same manner, with the exception that in A a high-calcium lime was used, and in B, a white or magnesian lime was used.

The differences shown in Table XII are very marked, the use of dolomite lime resulting in a loss in strength, both crushing and tensile, of more than one-third. Another noticeable fact is that the water absorption is greater in B than in A. Why the dolomite limes give a weaker brick is still an unsolved problem. Since MgO is at least equally active as CaO in attacking SiO_2, we can only infer that the magnesium silicate formed is a much weaker bond than the calcium silicate. This is also supported by the fact that there is also more combined water in brick made from sand and dolomite

lime than there is in those made from sand and pure lime.

C—PREPARATION OF RAW MATERIALS.

Now that we have a general idea of the requisites for satisfactory raw materials, the next step will be to determine what preparation they will need.

Beginning again with the sand, if our source of supply is a soft sand-rock, it will need to be crushed and the individual sand particles separated from each other. The machinery best adapted to this purpose will depend, to some extent, upon the nature of the rock to be crushed. If we have to deal with a sand obtained by dredging, it will usually be quite wet and must be at least partially dried, since a large amount of water is undesirable in the mixture. The degree of drying necessary will be dependent on the subsequent treatment prior to pressing.

On account of the presence of too much clay, or of soluble salts from sea water, as would probably be the case with sea-shore sand, it will be necessary to roughly wash the sand. In the first case the clay is floated off, and in the second case the salts are again taken into solution and in that way removed. The arrangements for accomplishing this will depend almost entirely on local conditions.

In any case, if the sand does not contain some very fine quartz, it will be necessary to pulverize a small portion of it. For this purpose a tube or ball mill can be used since they produce the greatest fineness. The Griffin mill will probably be found to be the one most satisfactory for this purpose. However, it will not be necessary to resort to any of this preliminary fine grinding in many cases.

In foreign countries it has been claimed by a few, to be advantageous to roast the sand. The advocates of this procedure claim that chemical combination is rendered more rapid and complete by giving the sand a preliminary roasting. I doubt if the activity of the sand is increased sufficiently to justify this additional expense. The exact value of roasting the sand remains yet to be determined.

Assuming that we have a comparatively pure lime, preferably a high calcium lime, properly burned, it must be hydrated, or in popular terms, it must be "slaked" either be-

fore adding the sand, or in connection with its incorporation into the sand. Owing to the slow slaking properties of magnesian limes, they could not be safely used unless they had been completely slaked prior to the addition of the sand. In the slaking of the lime, and in its incorporation into the sand-mix, lies the main difference between the various patented processes or systems in use. The essential feature of each system is generally the use of some special piece of machinery, or some special manipulation, for this purpose.

The slaking of lime is in one sense a very simple process, consisting in the addition of water to calcium oxide, or calcium and magnesium oxides, which takes the water into combination with it in certain proportions. One would hardly suppose that so simple and well known an operation should form the ground for the issuance of a large number of patents. Such is the case, however, both in this and in foreign countries. This is probably due to the fact that a chemical reaction is involved, and in many cases a reaction will not complete itself except under certain conditions. This is in a large measure true with the slaking of lime. Too much or too little water will yield an inferior product.

Most of the patents, however, are for methods or machines which utilize the heat generated in hydration, to dry out any excess of moisture and leave a product which is easily handled and stored without loss to its chemical activity.

While there are a large number of lime-slaking processes, which differ from each other in detail of manipulation, there are in reality only two distinct procedures; viz.,

1. The time honored method of slaking to a putty with a slight excess of water. This is used in the putty state, or after drying and grinding.

2. The other which slakes to a dry powder.

This is accomplished by adding such a quantity of water as will hydrate the lime but not so much as will leave the hydroxide wet. The excess must be so small that the heat generated by the hydrating of the oxides will expell it. The resultant product usually contains less water than that called for by the theoretical calcium hydroxide, yet there seems to

be no heating nor expansion on the subsequent addition of water for the purpose of using it in the arts or industries.

The method of preparing the lime will, in the nature of the case, govern the subsequent handling of the sand to a considerable degree, since in one case we have to deal with a plastic, tough, sticky mass, and in the other with a dry powder. However, the method of slaking should depend upon existing conditions, and that method selected which will best fit other parts of the process which have been decided upon for good reasons. In order to make the meaning clear a specific case or two will be cited.

Case 1. Wet Sand. If it was quite wet, we would dry some of it and add the unslaked lime to the balance, provided we had a high-calcium lime to work with. The moisture would then be corrected by adding dry sand and lime, or water, as the case might require.

Case 2. Moist Sand. Slake lime to a putty and incorporate. This has its drawbacks on account of the extreme difficulty of getting a small amount of tough sticky matter distributed evenly through a mass of crystals of quartz. I think it has one advantage, though I am not yet able to prove it, viz, that it will probably require less pressure in the machine to develop the maximum strength in the product, than is required where the material is mixed dry. I am inclined to believe the increased cost of mixing would more than cover the expense of drying the sand. If the lime is to be used in the putty state, it will always be found advantageous to work up the putty with all the water that is to be added to the mixture, thus getting it in a slurry or slip form, and in that way to facilitate distribution. But if there is much water in the sand, the amount left to be added to the lime is so small that it is of very little assistance.

Case 3. Dry Sand. This is the easiest to work, since the dry sand and dry hydrate, or the dry ground quick lime, can be readily mixed to a fairly homogeneous mass and the water then added and mixed in. The water will distribute itself to a considerable extent, if after the mixing the whole is allowed to stand in a bin or pile for a short time.

The facility with which a uniform mixture would be

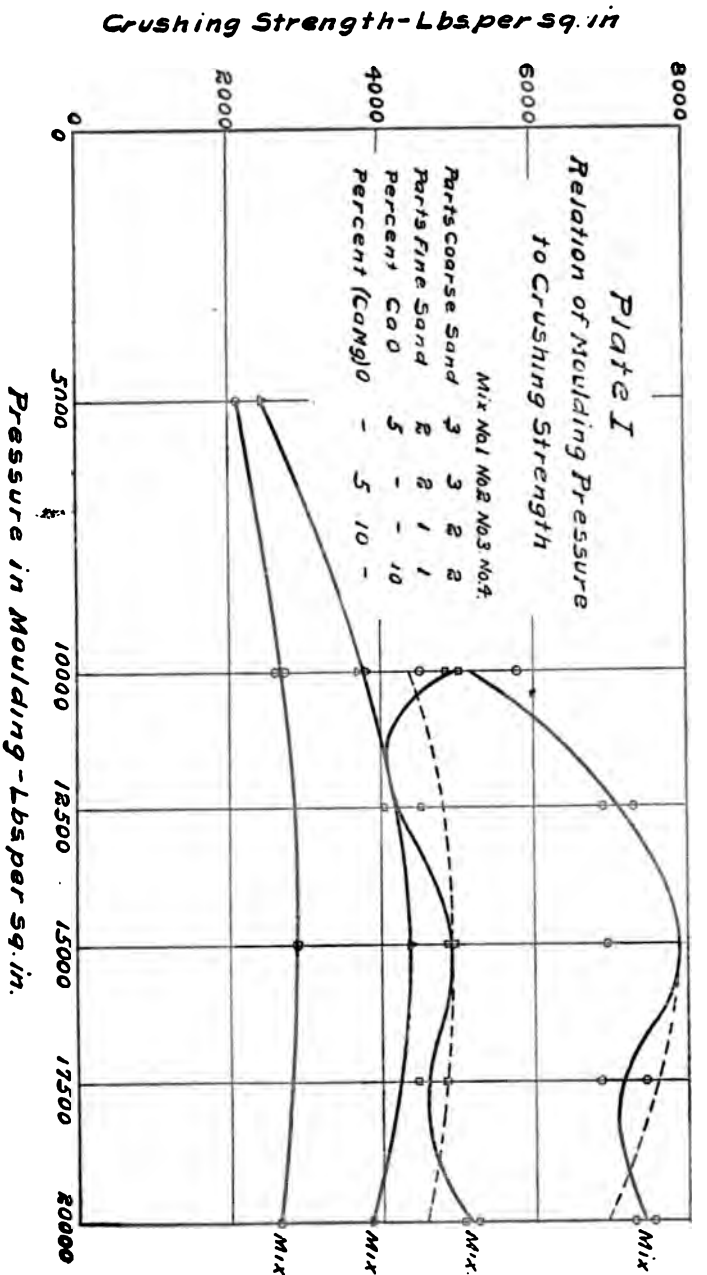
produced from the dry ingredients led us to adopt the third way as the means of preparation for all the materials used in our experimental work.

II—BEHAVIOR OF THE MIXTURE IN THE PRESS.

In the work done to determine what requirements a good brick press for this industry would have to fulfill, material prepared according to the third method, was used. The results obtained will therefore apply in the strictest sense only to that class of material. However, it is the opinion of the writer that any press which is satisfactory for this plan, will answer all the requirements for any of the others. It is the popular belief that the harder a brick is squeezed the harder it would be. But there is a limit at which the increase in strength in the brick would not justify the additional cost of construction and operation of the machine. In order to get some idea of this property of our material, we tried making a block on a hand power screw-press such as is used for making encaustic tiles, by giving it a number of pressures in succession. This block, after hardening, was crushed and compared with a duplicate made with one pressure only. This we found gave us a very decided increase in strength. This showed that we were not getting sufficient pressure, and since we were not able to control our pressure accurately, we abandoned the screw-press and adopted in its place an ideal brick press. This was a Tinius Olsen automatic testing machine, of 100,000 pounds capacity. On this machine we had the conditions of pressure, speed etc. under accurate measurement and control.

From the moment of inertia of the fly wheel, and the pitch of the screw on our hand tile-press, we figured roughly that we were getting from 5000 to 7000 pounds per square inch pressure. We therefore took 5000 pounds per square inch as a starting point, and made a series of blocks, using 5 per cent of each type of lime and the same sand mixtures, using pressures of 5,000, 10,000, 15,000 and 20,000 pounds to the square inch. All were hardened, at 150 pound steam pressure for ten hours.

The results of this work are plotted on Plate I. The


Crushing Strength-Lbs. per sq. in
Plate I
Relation of Moulding Pressure to Crushing Strength
Mix No.1 No.2 No.3 No.4.
Parts Coarse Sand 3 3 2 2
Parts Fine Sand 2 2 1 1
Percent CaO 5 - - 10
Percent (CaMg)O - 5 10 -
8000
6000
4000
2000
0
0
5000
10000
12500
15000
17500
20000
Mix No.4.
Mix No.3.
Mix No.2
Mix No.1
Pressure in Moulding-Lbs. per sq. in.

load at which the test-blocks crushed is shown on the ordinates and the pressure used in manufacturing on the abscissae. All represented in pounds per square inch. The two lower curves are the ones that illustrate this group. In both cases, the highest crushing strength is obtained at a die-pressure of about 15,000 pounds per square inch, with a slight falling off at 20,000 pounds.

Realizing the importance of this determination, it was though best to duplicate it, especially as the intervals were 5,000 pounds to the square inch. Although the curves seemed regular and pointed closely to 15,000 pounds as the maximum, it was thought that the correct pressure might be on one side or the other of that figure. A new set of test blocks were therefore prepared dividing the intervals. It was evident from the first curves that it was not necessary to go below 10,000 pounds to the square inch.

This set, as shown in the upper curves of plate I, began at 10,000 pounds die pressure, and increased by intervals of 2,500 pounds up to 20,000 per square inch. Ten per cent of a high-calcium lime was used in one set, and 10 per cent. magnesian lime in the other. The upper curve is the high-calcium lime and the second curve the magnesian lime. Duplicate cubes of each kind were made at each pressure.

Here again in the upper curve the highest point is over 15,000 pounds pressure. The figures are just a little lower for 20,000 pounds, and a marked depression occurs at 17,500 pounds. The second curve is a very irregular one, with 10,000 and 15,000 on a straight line, and 20,000 a trifle stronger, with depressions at 12,500 and at 17,500. Making a smooth curve, as indicated in both by a dotted line, taking 15,000 pounds as a high point, and averaging through, we get curves almost parallel with the two lower curves.

I should like very much to see a more extensive test made on this point, reducing the intervals of pressure to 1000 pounds per square inch, or less. However, I think we have sufficient evidence before us to say that the best pressure is close to 15,000 pounds to the square inch. It required about 75 pounds per square inch of surface exposed to the sides of the molds, to start a brick out of the mold,

when a pressure of 15,000 pounds per square inch had been given. The mold was made of cast-iron only. The pressure required to discharge the brick will be much less when the linings to the molds are made of hardened steel. This reduction in the friction will also slightly lower the pressure necessary in pressing. When lower pressures are used, the upper and lower portions of the brick are denser than the center. This difference is sufficient to be readily observed when using soft molds.

There is a good suggestion in this work for the dry-press brick-machine manufacturers. I should like to see some of them take up this point with reference to dry-press clay products.

The next consideration was the behavior of the loose powder of the mold. The same mixtures were used as in the proceding case, namely 2 parts of coarse and 1 part of fine sand, with 10 per cent high-calcium lime added in one case and 10 per cent. dolomitic lime in the other.

The curves in Plates II and III show the facts as taken from the Olsen autographic plate. Measurements were also made of total compressions, and length after pressure was removed. These were almost identical with that shown in the curves in both cases at the various pressures,, 5,000, 10,000, 15,000 and 20,000 pounds. The elasticity or expansion shown was greatest at 15,000 and 17,500 pounds, and least at 20,000 pounds. The difference was only from 0.01 to 0.02 of an inch.

The curves cannot show the recoil at any pressure, except the maximum, but do show the rate of compression for any pressure. The longest curve on upper plate was evidently due to not getting quite so much material in the mold, since it compressed beyond the point it should have done, giving a curve parallel to its duplicate. From the curves in Plate II, high calcium lime and Plate III, magnesian lime, it is evident the behavior is almost identical in both cases. Compression to 61.3 per cent. is accomplished with practically no load, and from that point the load increases very rapidly with each shortening of the block. The compression is less than 0.01 of an inch per inch

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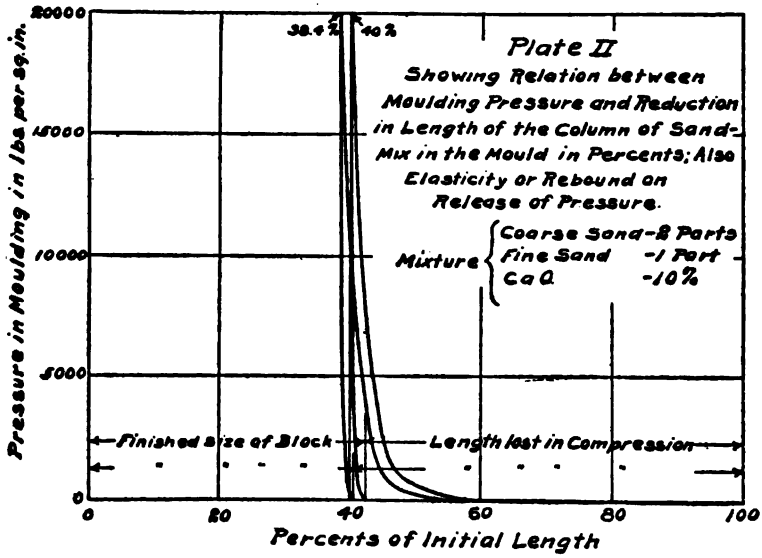

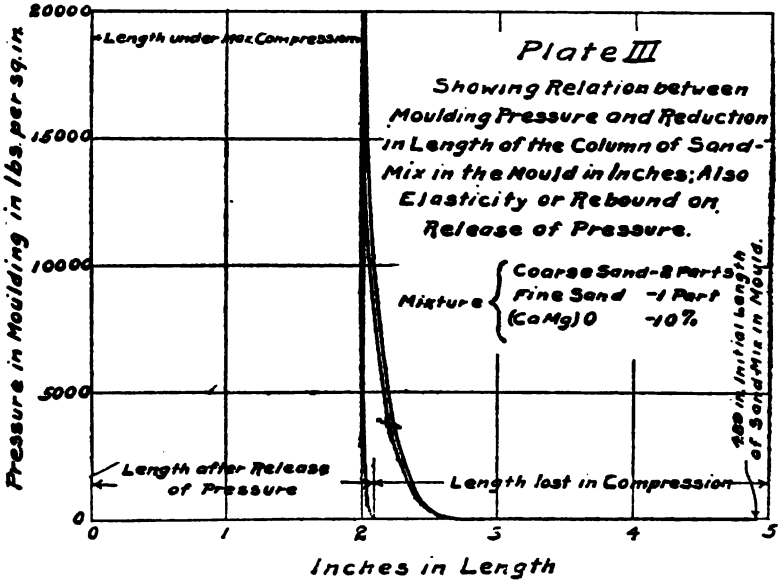

of length for an increase of 10,000 pounds, or from 5,000 to 15,000. The total compression was 40.9 per cent at 20,000 pounds, springing back to 42.9 per cent as final size.

For different mixtures of sand, the compression will undoubtedly be somewhat different, but will probably follow nearly the same curve, taking the load very rapidly after a pressure of 2,500 to 3,000 pounds is reached.

III—HARDENING.

In this connection I wish to urge on all prospective builders of plants, the necessity of careful and safe construction of the device for holding the head on the hardening kettle. Where accidents have occurred, it has usually been due to some defect at this point. The manner of fastening the bolts to the boiler should be carefully looked into, and no chances taken, since loss of life and destruction of property is sure to follow any carelessness in this direction.

It was known that good strong brick could be made in ten hours steam at 150 pounds pressure. It was not known how much less steam pressure would do, nor was the influence of the time factor known, and whether an extension of time would enable a material lowering of the steam pressure needed. To get data on these various points, the following tests were made as shown in table XIII. The pressures used in making the blocks were 15,000 pounds per square inch throughout.

(See Page 192.)

This table shows that 4 hours time at 150 pounds steam pressure is sufficient; that 6 to 8 hours are required at 120 pounds; that 8 to 12 hours are required at 100 pounds. The samples which contained 10 per cent of feldspar showed marked increase in strength in each case where the time was longest. I note this fact, since it suggests that long exposure to steam probably finally breaks up the feldspar, and in case it did so, might produce efflorescence. Knowing this fact, it would probably be desirable not to hold material containing much feldspar in the kettle any longer than necessary.

I should say general practice is to use 120 pounds steam

TABLE XIII.

Hours in Steam.	A		B		C		D		E	
	Crushing Strength.	Tensile Strength.	Crushing Strength.	Tensile Strength.	Crushing Strength.	Tensile Strength.	Crushing Strength.	Tensile Strength.	Crushing Strength.	Tensile Strength.
Steam Pressure, 150 Pounds.										
4	7896	544	5808	392	5282	591	4514	470	4441	380
6	7994	390	5045	199		....	....	....	....	...
8	7404	509	4957	262	6170	632	4249	480	4491	387
10	7767	464	4902	284	...	....	....	....	....	....
12	7514	337	5064	250	6165	556	4543	434	4924	349
14	7894	380	5849	329	....	....	....	....	....	....
Steam Pressure, 120 Pounds.										
4	6989	....	5989	....	5406	....	4800	....	5760	....
6	7063	....	6495	....	....	....	....	....	...	....
8	8545	....	6038	....	5868	....	5142	....	6718	....
Steam Pressure, 100 Pounds.										
4	6385	....	5921	....	4280	....	4048	....	4588	....
8	7566	....	6507	....	5564	....	4456	....	6544	...
12	7494	....	5753	....	....	....	....	....	....	....

pressure, and keep it on 8 to 10 hours, and in my opinion that is good practice. It usually takes about one hour to get the pressure up, and another to blow off and cool down sufficiently to be handled. The time required to raise the temperature will depend of course on the steam producing capacity, and the size of the inlet pipe, and the blowing off time depends on the size of the outlet pipe, etc. If the pipes are large enough and the boiler capacity is available, the temperature can safely be brought up very rapidly. I have put green brick in at a temperature of 130° C., and in about five minutes put on the steam, bringing it up to 150 pounds pressure in ten minutes more without any apparent injury to the brick. The brick on which I was working only carried 5 per cent moisture, however, and were small, viz 2 inch cubes.

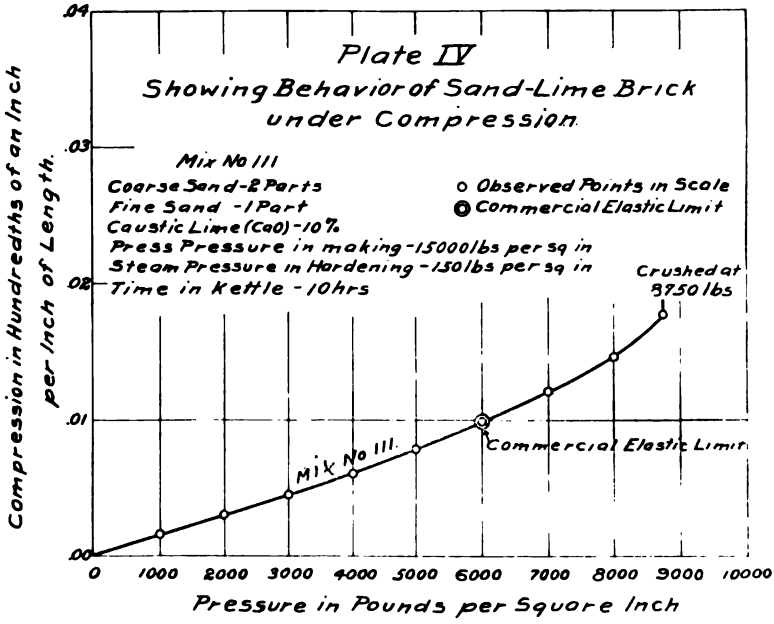

PLATE V.

CUBES OF SAND-LIME BRICK, SHOWING THE SHARP ANGULAR FRACTURE AFTER CRUSHING. IN THE LOWER ROW ARE CUBES OF COMMON MORTAR, SHOWING ROUNDED GRANULAR FRACTURE.

IV—TESTING.

By Chemical Methods.

The testing was in the main mechanical. However, the soluble silicic acid was determined in a number of cases, and enough results were obtained to show how much dependance could be placed in this determination as an indication of strength. It is merely a measure of the amount of bonding material.

By Mechanical Methods.

The mechanical test is always best, whenever it is feasible to make it. The results appeal more strongly to the public, even if the other were equally accurate, which is probably not the case, since the physical structure of the mixture has a decided influence on the strength, regardless of the amount of bonding material.

An autographic automatic Tinius Olsen testing machine, driven by power, was used for all tests. The autograph was only used on *types* of material, since more satisfactory and rapid work could be done without the autograph attachment on the machine. A ball and socket was used, as well as pasteboard to take up any uneveness of the block, on all except those on which curves were taken.

The crushing and tensile strength were determined in each test. Two to three blocks were crushed in most cases and the average reported. For the tensile strength, when two were pulled the average is given. Every brick made was tested, so that the results represent all the data obtained, not merely the best that it was possible to pick out on a single piece out of a dozen. Plate V shows a number of sand-brick cubes as they appear after crushing. The sharp angular cones and the splintery fracture show clearly that the material crushes exactly as a hard rock does. For comparison, a row of cubes made from lime mortars, which have been allowed to set and dry, are shown. The rounded outlines and low cones show beautifully the wide difference in structure and strength, though both sets of cubes are made of lime and sand.

A few of the brick made were set away in a dry room,

to try the effect of age upon them. These were duplicates of brick which had been tested, so that by comparison we could find out if there was any material change. There was not any marked change in strength, after forty days exposure to dry air, except in the specimen carrying 20 per cent kaolin. This showed considerable weakness. However, had these brick been moistened from time to time, they would have shown increased strength. This is due to the fact that CO_2 acts very slowly, if at all, on dry $Ca(OH)_2$. We should not place any importance on the above test unless it were carried out through several years, at least.

Resistance to Frost.

This test was made very severe. Duplicates of blocks which had been tested were used. These duplicates were made at the same time as the brick which were considered the standard, to enable us to see if weakness developed. About two weeks elapsed after hardening, before they were frozen.

The test was carried out in the following manner:—The lot of blocks to be tested were weighed; then dried at 110°C; weighed again; then soaked for 48 hours in water; then weighed; and the water absorbed figured as per cent of the dry weight. They were then placed, while still saturated with water, in a rack composed of wire netting and lowered into an empty ice-can, which was immersed almost its entire depth in the freezing brine at an ice plant. The temperature of the brine ranged from 12° to 15° F. The bricks were frozen to the centre in less than four hours, but were never taken out under that time. In this fully-frozen condition, they were lifted out, rack and all, and dropped at once into another can containing water at a temperature of 95° to 105°F, and thawed out entirely. The interval needed was less than two hours, but that time was always allowed. From here they were lifted dripping wet and dropped at once into the freezing temperature. This was repeated 21 times on all blocks tested.

As an experiment three cubes were put into the distilled water, which was then frozen solid. The ice was then

thawed and refrozen several times. The cubes showed no signs of breaking down. These were not tested.

The material after this severe treatment was further abused, by being dried out in a few days at a temperature of 125°C, after which it was tested. Many results have been given in the preceding tables in such a way as to be comparable with unfrozen material, and the facts have been, in a measure, brought out prior to this statement.

The blocks, almost without exception, as may be seen by going over the tables, showed marked increase in crushing strength, and in some cases increase in tensile strength as well. Usually the tensile strength was slightly less, if any material change had taken place. However, duplicates on the frozen material were not available in every case, and the results are not so conclusive on this account.

The reason of this increase of strength must be due to one of two causes:

1. The lime was not all combined with silica, and this uncombined lime took up carbonic acid from the water in which they were thawed out. This is very likely for two reasons. First, the water was drawn from a deep well near Columbus, Ohio, and I know from previous experience that these waters are rich with calcium and magnesium salts and also carry much carbonic acid in solution. Second, those specimens which were highest in lime showed greatest increase in strength after freezing.

2. In case the calcium hydro-silicate formed has hydraulic properties, which I doubt, those properties would be developed by constant soaking in water and would naturally develop more strength. I think, however, if the strengthening action were of a hydraulic nature, it would be most marked on the tensile strength rather than the reverse.

Resistance to Fire.

In order to determine the properties of sand-brick under fire, the following experiment was carried out.

Five blocks, representing five different sand-brick compositions were placed in a muffle kiln and the temperature brought up rapidly until Seger cone 6 was down. This cor-

responds roughly to 1250°C or 2282°F. Eight or ten hours was required to get this temperature. The blocks were taken out while still warm. All the blocks were perfect. The only change observable was that they were whiter, and had taken on a semi-vitreous lustre. There was neither fire-shrinkage or expansion. The blocks when cool were exactly the same size as when put in. Ten per cent of lime was used in each case, and both high-calcium and magnesian limes were represented, as well as ten per cent kaolin and ten per cent feldspar mixtures.

Not satisfied with this, the muffle was brought to a white heat, and five more blocks representing the five mixtures were put in on top of the muffle, and the heat continued to cone 6 or 8. These samples had but 5 per cent of lime in them. Before the opening to the kiln was entirely closed, all the blocks which had either clay or feldspar in them exploded. Those composed of pure quartz-sand and pure or magnesian lime, stood the test all right. They again showed the only change in greater whiteness and more porosity due to loss of combined water. They were not so hard as the first set, since with only 5 per cent lime the point at which the amorphous silicate would form was not reached. This shows clearly that brick made from comparatively pure quartz sands will stand a very high heat without injury, since even if the small amount of calcium silicate present were made fluid by heat, there would be no deformation, unless there was great pressure on them. The mass of sand particles built up and wedged in place would not require much to hold them in relatively the same position.

Nothing was done to determine the expansion while hot, but with low lime content it would not be far from that of pure quartz, and about the same as silica brick. Dr. E. Cramer states that a sand brick tested, the composition of which is not given, melted to a white glassy mass at Seger Cone 33, and that in a porcelain kiln, another specimen gave a linear expansion at cone 17 of 2.08 per cent. Further heating at same temperature gave an expansion of 2.72 per cent.

The figures quoted above were given in the last Convention of the Lime, Sand Brick Society, at Berlin, Germany.*

Since these brick are 90 to 95 per cent. silica or quartz, the heat conductivity must be very low, since quartz it is a much poorer conductor of heat than glass. Flint glass has a coefficient of heat conductivity 0.143 while that of quartz sand is only 0.0131.

Electrical Resistance.

For electrical resistance, 8 plates were tested. They were made 4x4x½ inches with a half spherical indentation in the center leaving a thickness approximately one-fourth inch in the center, where the electric terminals were applied. The rupture invariably occurred from ⅛ to $\frac{1}{16}$ of an inch from a line joining the two terminals of the instrument, showing that the current arced through the hydrous silicate bond, as would be expected. The voltage required to puncture the one-fourth inch thickness varied from 15,000 to 20,000 volts.

Comparative Properties with Other Materials.

With comparatively pure sands, the brick are white to grayish, depending on the sand. Small amounts of oxide of iron do not materially affect the color. The bricks may be colored red, black, yellow, brown or green, and these colors are not or subject to wide variations, *i. e.* by duplicating the mixtures, the resultant colors will be duplicated. In ordinary manufacture, there is great uniformity in color.

The brick are very slightly hygroscopic. The average of 20 determinations show that about 1½ per cent of water is retained in the normal air-dried sand-brick. The hygroscopic property is strongest when clay is present.

The water absorption is almost complete in 24 hours; in 48 hours the brick made by us took up from 8 to 10 per cent of water, calculated on the dry weight of the brick. The majority of the results were close to 8 per cent, as may be seen in the tables previously given.

*Thon-Industrie Zeitung, No. 81, 1908.

The following table gives a comparison of strengths shown by sandstone used for construction, and of sand brick.

TABLE XIV.

	Natural Sandstone.	Sand-Brick.
Weight per cubic foot.....	137 lbs[1]	136 lbs
Absorption.....	7.3%[2]	8%
Crushing Strength.....	6585 lbs[3]	7745 lbs[5]
Coefficient of Elasticity.....	165440[4]	600000[6]

1. Average of 16 samples, Bulletin Wis. Survey, No. IV, p. 402.
2. Average of 16 samples, Bulletin Wis. Survey, No. IV, p. 402.
3. Average of 51 samples, Bulletin Wis. Survey, No. IV, pp. 393-394
4. Average of 28 samples, Bulletin Wis. Survey, No. IV, p. 399.
5. Average of 12 samples tested by us.
6. Calculated from Curve No. 111 on Plate IV.

As you will note, the weight and absorption are very close. The crushing strength is in favor of sand brick as well as the elasticity. While the average strength of a large number of sandstones is less than that of sand brick, there are some sandstones of great strength, as is shown from the following record taken from "Brown-stones of Pennsylvania." A stone from White Haven, Pa., Appendix, 30, gave a crushing strength of 29,252 pounds per square inch.

The coefficient or rather modulus of elasticity is an arbitrary figure supposed to represent the theoretical load at which an inch cube would be pressed to zero length, if the block were perfectly elastic to that point. This figure is obtained from the amount or rate of compression, or shrinkage in length with increment of load, up to the commercial elastic limit, and the load at the elastic limit.

The commercial elastic limit is that point at which the compression curve increases by 50 per cent or more. An example will make this more clear. We will assume that with a load of 5000 pounds on a one inch cube, the shrinkage in length would be 0.01 inch; that is 0.002 inch for each 1000 pounds of load. Now, if when another 1000 pounds is added to the 5000 already on, the total additional shrinkage is 0.002 and 0.001 inches, or 0.003 inches, the commercial elastic limit would be 5000 pounds per square inch. The

high modulus of elasticity shown by sand brick means, of course, that they will make a very rigid structure.

This property of sand brick is perhaps made more clear by the statement that a sand brick cube with a load of 1000 pounds per square inch, only shrinks 0.00125 inch in length. Now since only 250 pounds per square inch is allowed to be placed on the best Portland cement mortar by municipalities, the greatest load that we could ask the brick to carry would only cause a compression of one forth that given, or 0.00041 inch per inch of height. This means that in any building, bridge or other structure which is called upon to carry a varying load, there will be little or no deformation due to this load. In other words very rigid structures may be made from this material.

The highest modulus of elasticity quoted in bulletin of Wisconsin Survey, No. IV, p. 399, is one from Alblemans and is 400,800. Limestones go sometimes as high as 1,800,000, and some granites above 2,000,000.

V—MECHANICAL EQUIPMENT.

The mechanical equipment of a plant consists of:—

(1) Power and transmission.
(2) Lime preparing machinery.
(3) Mixing apparatus.
(4) Presses.
(5) Hardening cylinders.
(6) Conveyors.
(7) Tracks and trucks.

Taking these up in order:

(1) Power and transmission will not differ materially from that in other industries.

(2) Lime-preparing machinery properly belongs to the lime industry, since if a hydrated lime is used it will probably be just as well to buy from the manufacturer, unless the lime is burned on the place, since the sooner the lime is hydrated after burning, the better will be the product. Those interested in this subject will find it discussed in the bulletin on the Lime Industry of Ohio, issued by the Ohio Geological Survey, in 1903. When quick lime is used, it

will be necessary to grind the lime, if bought in lump form. For this purpose either Stedman or Sturtevant mills may be used; either should be preceded by a small crusher.

Mixing Apparatus.

(3) A machine which might be mentioned in this connection, although it is not strictly a lime-preparing machine, is that A. G. Schwarz, of Zurich, Switzerland, in which the machine is heated by steam pipes, and the hydrate is dried by producing a vacuum. The excess of water is pumped out as vapor.* This machine is also used in the quick-lime process of manufacture, as a total preparing machine.

Other machines for mixing in either process are edge runners, wet and dry pans, and various mixing machines of the pug-mill type. I think the latter is the predominating type of machine in use. Any of the above named machines are efficient, if the mixing is continued long enough. Considerations of economy in power, durability of machines, available space, etc., should therefore decide on the proper machine for any particular case.

The Schwarz machine offers a means of always maintaining the moisture constantly the same in the brick before going to the press. When using other machines, the workmen soon acquire great skill in judging the correct moisture condition of the mix, so that with care there is but little trouble from this source.

Presses.

I think I am safe in saying there is no press made today which fulfills all the requirements of sand-brick industry. I do not wish to be understood as saying there are no American or European presses which will make good brick, because there are a number of them made on both continents. If we could combine the good points of each, and improve a few minor defects, we would have a very good press. The requirements which a good press must fulfill are these:

1st. The press must be able to regularly deliver a pres-

*"Silico—Calcareous Sandstones"—Ernst Stoffler.
See Also Engineering News, February 19, 1903.

sure of from 200 to 250 tons per each brick, and yet not break down if by accident the pressure rises somewhat higher.

2nd. The filling of the mold must be accomplished with great accuracy and uniformity.

3rd. All working parts must be so arranged that they will be free from contact with loose sand; otherwise, they will cut out at an alarming rate.

4th. The dies, and mold linings, must be made of the hardest material obtainable.

That a pressure of 200 to 250 tons per brick is essential to the development of good strength, was clearly shown in the discussion of the behavior of the mixture in the mold. Statements and results previously given also show that there is necessity for a press to stand much more than the normal load, since the strain on a press increases at a tremendous rate, with a slight increase in the amount of the mixture introduced, or any increase in its volume and weight such as might be brought about by some accident in the preparation of the mixture. The above also accounts for the necessity of accuracy in filling the molds. The lack of plasticity in sand, and its resistance to flow and adjustment of shape under pressure, is evident to all. Therefore, even distribution of the loose powder in the mold is essential.

The above refers to presses driven by direct gearing, in which the stroke of the plungers is fixed, and the pressure therefore directly dependent on the amount of material in the mold. This is the general type of machine in use. If the above requirements are not met, you can expect many break-downs in this type of machine. If, however, a press were constructed that put on a certain desired load and no more, or one which had a fixed stroke but also had some means of releasing the strain when it reached the danger limit, the result would be an occasional large brick perhaps, instead of an occasional broken press, with its consequent annoyances, expense and loss of time.

The third requirement is self-explanatory and evident to all.

The fourth is necessary to the reduction of friction and

more even distribution of pressure, as well as to save the annoyance and loss consequent to frequent changing of the mold linings.

The increase of press pressure increases the friction and abrasion of the mold linings and dies. It was found necessary when working with cast-iron molds, to raise the molds clear from the bed plate, so that the brick rested on the bottom die, so as to get pressure from both top and bottom. It is therefore likely that for the best results the pressure must come from both top and bottom.

It is probable that the friction on the sides of the mold, and the resistance to flow of particles will limit the thickness of a block which can be manufactured satisfactorily to four or five inches. Possibly a press may be so made that one charge may be added and given a definite pressure, and then another added and pressed on top of it; in this way, large blocks might be built up. The limiting factor here would be ability to push the block out of the mold without destroying it. Blocks made in this way would be expensive of course. I think the limit in thickness might be increased when necessary, by increasing the lime and moisture content. These would both tend to reduce friction, and increase movement of particle on particle, but at the same time increase the cost of production.

On account of the resistance of this non-plastic mass to flow, or the movement of particle on particle, it would be well that a press should not at once release the pressure given; or it might be better still to repeat the pressure. It seems that the hydraulic press might have some advantages, if adapted to this industry.

Considering the presses in use in this country, we can truly say they all have their merits, and if we were able to combine the good points of all in one machine, we would have a fairly satisfactory one. They all have their faults, however. One fault is common to all; they are not built to give the very high pressure which has been shown to be necessary. Since strength is developed in the brick either by giving it high pressure in the press, or by increase of the lime content, it follows that brick of equal strength can be

made with much less lime, if the press pressure is high enough, and it is probably cheaper to give this pressure, than to use larger amounts of lime.

Aside from this common fault, each press has its own weak points. Taking the Kahl press, which is a German press, soon to be manufactured with some improvements by the Ohlemacher Brick Company, of Michigan City, Indiana, we have a machine built to stand 150 tons per brick, and one which seems to do very good work up to about 100 tons, or a little more perhaps. This machine is built broad and low with the wearing parts well protected from the sand. The floor space of 1,000 bricks capacity is very large. This is always a disadvantage. The press consists of a rotary table, which first passes the empty mold under a special measuring device, which has the loose material already measured, and which drops the sand when the mold is in the proper position. The table rotates about 135° farther around and the brick gets its pressure from below, this portion of the table now being under a very heavy overhead arm. The table next carries the brick out from under the arm, after which the brick is pushed up flush with the table and may be picked off the head of the die without any scraping, or spalling. The pressure is delivered directly by an eccentric, working under the table. This is a weak point, which is to be remedied in all new machines. This is probably a weak point in any machine called on for enormous pressure. This machine when seen in operation by the writer was giving brick with clean-cut edges and corners, with almost no failures. One size has a capacity of 12,000 to 14,000 per day of ten hours, and the other, 20,000 to 23,000.

The Komnick press, another machine of German type, is used by the American Sandstone Brick Machine Company, of Saginaw, Michigan. It is an upright machine, with a smaller rotating table, and filling device similar to the Kahl press. The method of delivering the pressure is different, being given from above. This machine is supposed to give a pressure of about 100 tons per brick.

The Kahl press only makes one brick at a pressure; the Komnick, makes two. It occupies a little less floor space

than a press of the American type having twice the capacity. It has a capacity of about 10,000 in ten hours.

A weak point in this machine is that some of the wearing parts are not protected from the sand. When seen in operation by the writer it was not giving brick with clean edges. This fault was not inherent in the machine, but was probably due to the setting of the mold linings, and the condition of the die-heads.

This firm intends manufacturing a press of this general type with some improvements, making it double-geared and simplifying it at several points. This is to replace the other press in all their new plants.

Since sand-brick, when first pressed, are so tender that they do not stand pushing around and handling without injury, the rotating table is the one thing characterizing the German press which recommends it, and it was the result of an effort by the German manufacturers to adapt the press to the need of the industry.

The various American dry-press brick machines used in the clay industry are too well known to require any description. So far as known to the writer, they are the only machines of American construction yet applied to the making of sand-lime brick. They have greater strength, greater durability and greater capacity than any other. They have, however, one serious fault. The brick is pushed off the head of the die by the mold charger in such a way that many sand-brick are injured by having corners broken off, or the edges made ragged by little spalls breaking out. Two American brick-press companies think they have solved this problem, and will soon have new machines out. It is to be hoped that their anticipations may be realized. It will not be long, at any rate, until the problem is solved by some American mechanic. The writer favors the use and development of an American press. Even if many defective brick must be thrown back for a time, the American press will make more brick with less expenditure for labor and repairs than any other, and its use will soon lead to the over-coming of its present defects. It must be borne in mind, that it was

not designed for sand-brick at all, and it could not properly be expected to do this work without adjustment to it.

Hardening Cylinders or "Kettles."

(5). Hardening cylinders were described by the writer in a previous paper[1]. The following notes are supplementary to what is there given.

The head should be handled by an overhead crane or a block and tackle. Hinged doors with a wheel on the bottom, and a track for it to run on, have been used. These are clumsy affairs to move and occupy much space, and should never be recommended. The bolts should be so fastened to the kettle, that the head can readily be swung into place. These bolts are usually 1¼ to 1½ inches in diameter.

The size of these hardening kettles varies from five feet six inches to seven feet in diameter, and from 35 to 67 feet in length. They should be constructed of ⅝ inch to ¾ inch iron or steel plate. There has been considerable discussion among the German manufacturers as to the size of kettle which is best. From these discussions, I would infer that the majority are in favor of the larger sizes. The friends of the smaller size put forward the argument that the brick should be put into the cylinder at once, and that with the larger ones they would have to stand twice as long. This is true, but the experience of the writer would indicate that the difference in time of stand, which would be only a few hours between pressing and steam treatment, does not materially hinder the development of the usual strength. When working brick very moist, the danger of damage from standing before steam treatment, is increased, since the more moisture present the more rapidly will the free calcium hydrate take up (CO_2) carbon dioxide.

With the small cylinder, the cost of both labor and fuel will be slightly increased, as has been shown by Thiessen[2].

The size of the kettle which will be best, will depend on 1st, the plan of operation adopted at the plant, and 2nd, on the capacity of the plant, which means the press capacity.

1 Transactions American Ceramic Society, Vol. IV.
2 Thon-Industrie Zeitung, Vol. 26, No. 104.

With a press capacity of 20,000 brick in ten hours, I should recommend working with units of 10,000 brick, and should build each kettle to carry that number. If, however, the press capacity were only 15,000 per ten hours and the plant was to be operated only through the day, it is obvious that kettles of 10,000 capacity would not be convenient or economical. The customary size is six to seven feet in diameter by fifty to sixty feet in length. This size will, in my opinion, be not for from correct in most cases.

Tracks and Trucks.

(7). Of the trucks, nothing need be said except that they should be well made, strong, and easy running. No trucks should be used without ball or roller bearings, so that the power used in moving them will be small. The tracks also should be first-class, laid with rails of good weight and kept in good alignment, to avoid jolts or jars on the brick, especially before hardening.

VI—DISCUSSION OF THE MERITS OF THE VARIOUS SYSTEMS, INCLUDING PATENTS.

Five systems or processes of manufacture have been exploited in this country. Three of these have plants in operation today.

The American Sandstone Brick Machinery Company, of Saginaw, Michigan, are putting in plants after the system used by F. Komnick, in Germany. This company has one plant up. The Ohlemacher Brick Company, of Michigan City, Indiana, represent the Kleber system as patented in the United States. This company has the pioneer plant in the United States.

Mr. H. E. Brown, of Coldwater, Michigan, is exploiting a patent on the use of *dolomite lime.*

The Schwarz System Brick Company, New York, has the Schwarz system to offer.

H. Huennekes & Company, of New York, are exploiting the Huennekes system, which is based on the United States patent of Joseph Prior. This company has three plants in operation today.

Patents on Hardening Processes.

There are quite a number of patents which bear directly or indirectly upon the sand-brick industry as defined by us. There are not many, however, that have real merit. *There is no patent on the process of hardening with steam under pressure in the ordinary way, since Dr. Michaelis, of Germany, the originator of this idea, allowed his patent to expire some years ago, thus giving it to the public.*

However, there have been granted in the United States two patents on hardening processes, which bring about the same reaction and essentially by the same agent—hot steam or steam pressure. While both of the processes will harden the brick, I cannot see where they introduce any advantage or economy over the old way of doing it; in fact, I think there are several factors introduced, which would bring about a loss of heat, in place of economy.

These patents are:

Patent 681580, August 27, 1901, granted to *Carl Rensing*, Berlin, Germany.

Patent 683337, September 24, 1901, granted to *Walter Schulthess*, Zurich, Switzerland.

An electrical hardening process has also been patented.

Patent 661443, November 6, 1900, granted to *Emery Coulon*, Blatson, Belgium.

But the latter, as outlined in the specifications, would probably be too expensive to receive any commercial attention in this country. So far as I have been able to find out, none of these three patent hardening processes have received any commercial impetus.

Patents Covering Objectionable Procedure.

In the United States, there have been granted between January 1, 1892, and September 30, 1902, twelve patents, which bear directly upon the sand-brick industry. Three of these have objectionable features, considered from the point of material produced. Good brick can be made by any of the other nine. The question simply resolves itself into one of economy of manufacture, so far as they are concerned.

Patent 693906, February 25, 1902, granted to *Edward*

Rott, of Cassel, Germay, covers mixtures of clay and lime hardened in steam. The quality of material produced by clay and lime hardened by steam has been discussed earlier in this report, and needs no further comment.

Patent 685094, October 22, 1901, granted to *Sven Edmund Boirie*, of Rognhildsborg, Sweden, makes use of nitric acid, in the mixture. This is almost sure to leave soluble salts in the finished product, which would be very undesirable.

Patent 470333, March 8, 1892, issued to *Charles George*, of Berlin, Germany, gives the brick, after steam hardening, a bath in a solution of calcium-chloride. If he were to forget to give his brick their bath, they would be more desirable.

Patents Covering Impractical Procedure.

While good brick can be made under the next three patents, I do not think they are feasible commercially.

Patent 624900, May 16, 1899, granted to *Thomas Barber*, London, England. I will quote claim No. 1 for this method. "The process of manufacturing artificial stone, which consists first in submitting the materials to a vacuum to exhaust the air therefrom, then submitting them to the action of hot water under pressure, then submitting them to the action of steam under high pressure and removing the water, then again submitting the materials to a vacuum while maintaining them at a suitable temperature till dryness is reach, substantially as described." The brick must stay in the mold during this entire procedure.

Patent 591168, October 5, 1897, granted to *Christian Heingerling*, of Frankfort-on-the-Main, Germany. There are three claims: claim 1 would make good brick, but they would have to remain in the mold while being subjected to vacuum after molding; claim 2 dips the brick in oleagenous substances, which would be objectionable for most purposes; claim 3 hardens by carbonic acid gas.

Patent 707898, August 26, 1902, granted to *Teodor Boas*, of St. Hyacinthe, Canada. This patent covers the use of magnesium-silicates with lime, and steam hardening. There are eight claims. I am not prepared to say what the results would be for the first four of these, but with the bal-

ance, where sand and lime are used with magnesia in excess of the lime, I think a good brick could be made. However, there are not many locations where magnesia or magnesium-silicate can be had cheaper than sand. Since probably the only office of the magnesia compound would be to replace sand, it would do just as well to use cheap sand.

Patents Covering Practical Procedure.

Patent 684649, October 15, 1901, granted to *Wilhelm Olschewskey*, Berlin, Germany. The only new feature introduced in this patent, is the heating of a portion of the sand mixed with lime in a closed vessel. The balance of the sand and water are added later to cool the entire batch. The additional manipulation here, of course, will slightly increase the cost of production. It is necessarily an intermittent process. Since brick are made to answer all requirements without this extra heating and manipulation, this process is not likely to become popular in this country.

Patent 702611, dated June 17, 1902, application filed December 4, 1901, granted to *Oscar Hugo Anderson*, of Stockholm, Sweden, assignor by mesne assignments to the Silicate Brick Syndicate, of Montreal, Canada, a corporation of Delaware. The essential feature in the claims of this patent, is the slaking of lime in dry high-pressure steam *at the same time*, and *in the same hardening cylinder* as the bricks are hardened in. The principle of slaking lime in dry steam is not new, since Walters published the properties of lime prepared or hydrated in dry steam a number of years ago. The novelty of the patent must therefore lay in the slaking at the same time the brick are hardened.

The inventor fails to call attention in his specifications to the most valuable feature of the entire process, namely, the fact that all the heat of the chemical reaction of the water on the lime is utilized. The heat generated is considerable. And all the heat given off by the slaking lime in the hardening cylinder means that just so much steam is saved from condensation by radiation or in raising the temperature of the brick and the trucks up to the temperature of the steam. The passage of the steam through the hardening

cylinder with sufficient rapidity to keep it essentially free from moisture, would be an expensive operation. It is not necessary, however, that this be done, since moist steam will do no harm so long as the temperature is maintained.

The practice recommended in this patent, with the exception that the lime is practically slaked by water before being introduced into the hardening cylinder, is represented in this country by the practice of the American Sandstone Brick Machinery Company, of Saginaw, Michigan.

In April 1900, F. Komnick, of Elbing, Germany, issued a pamphlet "Sandsteinziegel-Industrie," in which he shows a container for slaking lime under high pressure steam, at the same time that the brick are hardened.

At Saginaw, Michigan, there is a very neat little plant, 12,000 to 14,000 in twelve hours, operating after the system of Komnick. This plant has some minor faults in its arrangement, recognized by the owners. These will be corrected in all future plants. The procedure at this plant is essentially as follows:

The lime is prepared by adding water to quicklime in iron boxes of such size as will go under the brick trucks in the hardening cylinders. After the addition of the water, the boxes are allowed to stand a short time and are then placed in the hardening cylinder. Any lime unslaked by the water is fully hydrated by the steam during the hardening of the brick. If, on the other hand, a little too much water is added, the steam drys the hydrated lime. This gives a very satisfactory lime-hydrate to work with. There are always some lumps which do not entirely break down, so the hydrate must be ground before mixing with the sand.

The ground lime-hydrate and sand are shoveled into an elevator, which carries it to the mixing machine. This machine consists of two shafts studded with paddles turning towards each other. The paddles are so set as to gradually carry the mixture to the other end, which feeds into the press. The press used, has been described. Short hardening cylinders (27 feet) are in use here. This is not an essential feature of the system, however. My observation of the material produced, was that the lime was not evenly distrib-

uted. This must be due to one of two causes, *i. e.*, either insufficient grinding of the lime, or insufficient mixing later. Both finer grinding and a second mixing machine would be perhaps desirable.

With almost any other method of preparing the lime, the imperfect distribution of the lime would be dangerous to the product, but with this method of preparation, the only undesirable feature is that the full value of the lime as a bonding material is not obtained. This system of manufacture evidently will not be the cheapest, since the users of quicklime eliminate the cost of the preparation of the lime as indicated above.

Patent 697391, April 8, 1902, granted to *Herman Elisha Brown*, Coldwater, Michigan. This patent covers the use of lime produced from the calcination of dolomite limestone. There are no plants working on this system today, but the contract has been let for the machinery for one to be put up at once at Jackson, Michigan.

As previous statements would indicate, this type of lime must be thoroughly slaked before mixing with the sand, and more time will be necessary for slaking than with hot limes. The mixing machine contemplated in the new plant, is one which has been used in mixing asphalt and sand. Since it is a machine which holds the material until dumped, it should give good results if the mixture is retained until the lime is completely distributed. The hardening cylinder contemplated is extreme in length (76 feet). In my opinion this is a mistake, for any thing except perhaps, a plant of 150,000 to 200,000 brick per day. The back end of such a cylinder will have very bad light. And further, it will be very difficult to keep such a long cylinder from creeping on the foundation.

This process obviously will not be the cheapest one since, as has been previously indicated, slightly more of this lime is needed to produce the same strength as of a hot lime. However, there are no doubt locations where this kind of lime can be had at a sufficiently lower cost to more than over-ballance any *slight* drawbacks which it may have.

Patent 686333, November 12, 1901, granted to *Paul Joseph*

Prior, of Cologne, Germany, assignor to Heinrich Hünnekes, of Straelen, Rhineland, Prussia, Germany. The essential feature of this patent is the introduction into the hardening cylinder of the alkali salts, potassium and sodium carbonates, in such a way that the steam introduced shall pass over the surface of the salts, claiming that in this way potash and soda are introduced into the brick. This patent has been exploited in this country and is better known under the name of the "Huennekes System."

The properties of potassium and sodium carbonates, are such that it is difficult to see how they can be carried up and into the bricks to any extent by the steam. After seeing a plant at Pittsburgh, Pennsylvania, which operated according to this system, the writer concluded that the alkali salts were dissolved and carried out by the water which is inevitably condensed in hardening the brick by steam. This being the case, the alkali salts could do neither harm nor good, so long as the steam inlet and the salts were kept under the trucks which carry the brick. If, however, the alkali salts were suspended over the brick, and the steam then blown over the salt containers, there might be positive harm done by the mechanical action of the steam, splashing the solution of potassium and sodium carbonates over the surface of the brick. These salts would later be apt to show up as a white scum on the brick.

Owing to the fact that three plants were working in this country under protection of this patent, and that the promoters of this system were making rather strong claims as to the desirability and advantanges of this system over all others, it was deemed advisable to make an investigation of the advantages, if any, to be derived from the use of potash and soda.

Accordingly, a mixture of four parts of the coarse sand used in earlier work, and one part of fine-ground flint, and ten per cent. high-calcium lime was made up, and three cubes and three brickettes made from it with a press pressure of 15,000 pounds per square inch. These were hardened in a hardening cylinder, in which the steam inlet deliv-

ered the steam over the surface of potassium and sodium carbonates exposed in shallow pans. This was accomplished by closing the end of the steam delivery pipe which was laid on top of the shallow pans. This pipe had a large number of holes on both sides of it so that the steam came out all over the surface of the salts. The cylinder was heated above 100°C before putting in the bricks and the alkali salts, so as to have as little condensation as possible, but nevertheless the pans were filled and running over with water when the cylinder was opened. The average results obtained from crushing the three cubes and pulling two of the brickettes was:

Crushing Strength, pounds per square inch	6,181
Tensile Strength, pounds per square inch	554

Three more cubes and three more brickettes were then manufactured under strictly parallel conditions, except that *no alkali salts* or *chemicals* were used in the hardening cylinder. The average results obtained from crushing the three cubes, and pulling two brickettes were:

Crushing Strength, pounds per square inch	6,415
Tensile Strength, pounds per square inch	644

From the above results, it seems evident that the introduction of potassium and sodium salts into the hardening cylinder in the prescribed manner brings about no material *good* or *injury* to the brick. We may say then, that it occasions a slight increase in the cost of production, without any material gain in the quality of the product.

As has been stated earlier, there are three plants operating under the protection of this patent. One at Pittsburgh, Pa., one at Wilmington, N. C., and another at Sioux Falls, South Dakota.

The general practice at these plants is to mix ground lime and sand with the requisite amount of water to produce the proper consistency in a pug mill. The mixture is conveyed to bins where it is allowed to ripen for 24 to 48 hours, after which it passes through a second mixing machine of some sort, and on to the press. The Berg press is in use at Pittsburgh. The second mixing machine at the above named

place is a dry-pan, and by this procedure fairly even distribution of the lime was being obtained. A rather large hardening cylinder was being used here also—about six feet six inches in diameter by 66 or 67 feet long. I can see no reason why this mechanical treatment should not be slightly cheaper than the two preceding ones.

Patent 670299, March 19, 1901, granted to *Wilhelm Schwarz*, of Zurich, Switzerland. I will quote claim three which gives the essential features of the patent.

"The process of making artificial sandstone, which consists in drying the sand by heat *in vacuo*, then adding lime to the dry sand, and drying the lime, then mixing the whole and adding moisture all while under a vacuum."

The main object of this patent is to bring about accurate control of the moisture content of the raw brick. While this is desirable, I do not consider it essential, since my observation is that the workmen soon acquire sufficient skill to keep the moisture content of the mixture within the limits which will produce uniformly good brick. The *very small* amount of carbon dioxide which is eliminated by the machine being closed while mixing is in progress, is not worthy of any consideration.

In the opinion of the writer, the real merit of the Schwarz system lies in the mixing machine which Schwarz has devised to carry out his ideas, rather than in the patented process. This machine is described in Engineering News for February 19; 1903.

If this machine were divested of its complex vacuum and heating arrangements, I believe it would then be the best mixing device for the sand-brick industry which has yet been offered to the public. Since the total preparation of the raw material is brought about in one machine, it admits of simple plant construction, eliminating in a large measure conveying machinery which is a very desirable thing to do.

The Schwarz System Brick Company, of New York, are promoting this system, but as yet have no plants in operation.

Patent 663904, December 18, 1900, granted to *Peter*

Kleber, of Malatatt-Burbach, Germany. I quote the entire claim made by this patent.

"The herein described process for the production of artificial stone or brick, consisting in first mixing moist sand and unslaked lime so as to hydrate a portion of the lime only; second, adding sufficient water to hydrate the lime remaining unhydrated or unslaked, and allowing said mixture to stand sufficient to complete hydration; thirdly, bringing the mixture into a plastic cohesive condition by the addition of a minimum amount of water; molding said mixture into blocks and finally subjecting such blocks to the action of high pressure steam, substantially as described."

A careful study of this claim will show you that there is very little in it which is essential to sand-brick manufacture. No new principle is involved. Nothing can be gained by furnishing the moisture to the lime for hydration in two portions, instead of one; in fact the reverse is the case, since lime slakes best and most completly with a slight excess of water, such as would be needed to bring the entire mixture to the desirable condition for molding, after the lime had taken up what water it would. With some limes which are high in magnesia, it would be necessary to let the mixture stand at some stage after the addition of the water necessary for hydration, to give the slow-slaking magnesia time to complete its hydration before going to the press. With a hot, quick-slaking, or high-calcium lime, this would not be necessary; the hydration would be complete by the time the mixing had been thoroughly done, and the mixture could be put through the press and into the hardening cylinder while still warm from the heat generated from the slaking lime.

The Ohlemacher Brick Company, of Michigan City, Indiana, originally manufactured under the protection of this patent, but I do not believe in their present practice any just cause for action for infringement on the above Kleber patent could be found, were they inclined to abandon its protection.

The Michigan City plant is the pioneer plant in this country. The practice at this plant briefly stated is, to mix sand, ground quick-lime and water sufficient to completely slake the lime, and leave the mixture moist enough to mold

when finished. This is done in the first mixing machine. The mixture then passes through two more mixing machines, and screw conveyor, before going to the press. If the moisture content is not right, it is corrected in the last mixing machine by adding a little water, or a little dry lime and sand as the case may be.

The mixing machines are of the pug mill type, with many more paddles than are in an ordinary clay pug mill, The distribution of the lime was complete. The raw mixture is carried to a row of presses by an overhead drag conveyor, which has feed openings at each press, which can be closed when a press is not in use. The hardening cylinders are of normal size.

A properly equipped plant operated according to the last patent, or in the manner just described, should produce brick at minimum cost, since there is no extra handling of anything. The raw material starts at one end and comes out a finished product at the other end, with no delay except for the steam bath. A considerable portion of the heat, due to hydration of the lime is saved. Any other than a quick-slaking lime would make trouble however.

Summary.

I wish it to be understood that good brick can be produced under all of the last nine patents described, and that the value of each lies in its ability to reduce the cost of production, rather than in any advantage in producing the chemical reaction in hardening.

If the last five systems described, were all to work under exactly parallel conditions, I do not believe that the difference in cost of production would vary more than 35 cents per thousand, between the lowest and the highest. Likewise, the difference in cost of construction of the plants for each process will not vary more than a few thousand dollars under similar conditions.

The cost of plants will, of course, vary slightly from time to time with the rise and fall of hand and machine work, but will vary markedly with the locality at any time. A well equipped sand-brick plant, with a capacity of 20,000 bricks

in 10 to 12 hours, will cost, independent of site, from $20,000 $25,000 in Ohio.

The cost of production also will vary considerably with location. The cost of production in this country, independent of depreciation and interest on investment, varies from $2.50 to $4.00 per 1000. The selling price ranges in different localities from $8.00 to $15.00 per 1000.

From seven to eight million brick were made and sold in the United States in 1902.

It is a fact worthy of notice that of all the patents mentioned, only one was taken out by a citizen of the United States.

I see no valid reason why plants should not be built, or why good brick should not be made, independent of all the above mentioned patents or systems.

DISCUSSION.

Mr. Alfred Yates:—I think the gentleman who has presented this paper deserves credit for working it up in such detail. But, it is an innovation to expect a clay-worker to make brick from sand. I have heard no reference to the money end. It is the commercial end we are all looking at. If we cannot make money out of an industry, we naturally will not take hold of it. I notice Mr. Peppel recommends a press operating at fifteen or twenty thousand pounds per square inch. We have presses in this country running at eight or ten thousand pounds, and we think these are excessive pressures. I would like to ask him if he expects to install presses to work this increased pressure and still make a commercial success?

Mr. Peppel:—Certainly. Why not? I do not recommend more than fifteen thousand pounds to the square inch, and I should say that sand-lime brick presses now in operation are using between twelve thousand five hundred (12500) and fifteen thousand (15000) pounds per square inch. I don't believe anybody in the clay brick line is manufacturing with over seven thousand five hundred (7500) pounds to the square inch.

Mr. Yates:—We all know that clay is compressible. It

will reach a certain density, and when we arrive at that stage, it is elastic. But in my experience, whatever press may be used, there must always be a little margin left, so that if we get a little more clay in the mold, it will not break the machine. We have little vent holes to take care of it. What are you going to do, when you get a little more sand between the plungers than the space will admit?

Mr. Peppel:—That point is just what I brought out in my paper. I showed that the compression is very small beyond fifteen thousand pounds per square inch. Therefore, if you get a very little sand in the mold beyond that which is required to fill it at the normal pressure, you run up the pressure at an enormous rate, and possibly break the machine. This is a point which our American machine manufacturers have got to meet squarely before their presses will succeed in this new industry.

Mr. Yates:—I would like to ask, can this mechanical combination be brought into such dense form that this brick will be impervious to moisture?

Mr. Peppel:—Oh, no! The average absorption of moisture is about eight per cent.—it ranges from six to ten.

Mr. Yates:—The samples of this sand-lime brick which have been handed around here are very much like silica brick. I know that if a silica brick freezes, it is atomized. How do you expect these brick to stand frost, with eight or ten per cent of water in them? I am asking these questions because if there is more money in sand-lime brick than in clay brick, we want to know it.

Professor Edward Orton, Jr.:—Mr. Yates did not seem to gather during the reading of this paper, that Mr. Peppel made freezing and thawing tests 21 times in succession on the same bricks, and that after that treatment they actually stood more pressure than a similar set of bricks which were not frozen.

(Mr. Yates shakes his head, looks doubtful, and finally bursts into incredulous laughter. The humor of the situation strikes the audience, and general laughter results.)

The Chair:—There are one or two successful plants operating in this country on this line?

Mr. Peppel:—There are in this country five plants in operation, and machinery is bought for another one. In Germany there are several hundred plants in operation.

The Chair:—I assumed naturally that Mr. Peppel would not have taken the time to prepare all of this elaborate investigation on an up-in-the-atmosphere proposition. I gave him credit for that.

Mr. Yates:—I would like to ask the gentleman, if he knows whether any of these American plants have paid any dividends?

Mr. Peppel:—I am not familiar with the business management of these corporations, and do not know whether any of them have paid dividends or not. But the question as to whether they have paid dividends or not is premature, as they are all new and introducing a new product. We know what it costs to manufacture sand-lime brick. We know what they can be sold for; know what a plant costs; it is simply a question of ability to organize the business and to sell the product. They cannot help but make money, if they receive ordinary sound business management, and are operated by a man who knows the technical side thoroughly.

Professor H. A. Wheeler:—What are the figures?

Mr. Peppel:—I gave these figures a year ago in my first paper before this society, and a year's study of the question does not show that they ought to be changed. I thought then the cost of the brick would be approximately $4.00 per thousand. The figure I gave for the cost of a plant, independent of any royalty, was approximately fifty thousand dollars.

Professor Wheeler:—Will sand-lime bring the same prices as common building brick?

Mr. Peppel:—In different sections they are being sold at different prices. At one point they are sold at a little better price than common brick; at another place they are sold just a little bit under face brick. Their appearance would justify their going into the market about on a par with face brick. However, they can be made cheap enough to sell at par with common brick. The price they will sell at is a matter for the people going into the business

to decide for themselves. Eighty-five to ninety-five per cent. of the brick *can* be sold as face brick; the other part would simply be face brick with the corners knocked off, etc., in handling.

The Chair:—Have any abrasion tests been made?

Mr. Peppel:—No abrasion tests have been made. The abrasion test is never applied to building material so far as I know. It is used for comparing the toughness of material which is made to withstand wear, like paving brick. However, the tensile strength suggests in a measure, how the brick will wear in handling.

The Chair:—Were any of these bricks tested after the absorption test was made, while wet, to determine suitability for foundation work, where they would be constantly under water?

Mr. Peppel:—No, not by me, but some tests have been made in Germany, on the wet material, which show very slight differences; about the same proportionate difference which a clay brick usually shows when tested wet vs. dry. I don't mean vitrified brick, but ordinary building brick. We are not advocating sand-lime brick for pavers.

Professor Wheeler:—Have any attempts been made to color these brick?

Mr. Peppel:—They have been colored. This (showing sample) was made in an accidental way, and is not very beautiful.

Professor Wheeler:—Have any heat tests been made?

Mr. Peppel:—They have been heated to cone eight.

The Chair:—Have they been tested as a fire brick?

Mr. Peppel:—Not by me. My test merely consisted in placing them in the kiln and carrying them up to cone eight.

The Chair:—In direct contact with the fire gases?

Mr. Peppel:—They were in direct contact with the gases, but were not in the fire box. The Germans have made a number of tests. They built a small house out of the material, and built a wood fire around it, making a small sized conflagration; they claim that they came out in better shape than common brick. But I do not lay much stress on that test.

You cannot use this material as fire brick in a position where basic slags can come into contact with it. You can only class it as a fire-resisting material in such places as an ordinary silica fire brick would be desirable.

Professor Orton:—Do you think it will stand up with ordinary silica brick? Does it not contain too much lime?

Mr. Peppel.—It can be made with two per cent. lime. What is there to make it weak?

The Chair:—I had in mind the lining of muffles. I wondered if it could be used there.

Mr. Peppel:—This block (picking up one from the table) contains five per cent. of lime, and has been burned, but not up to the point where vitrification sets in. I think it was about to cone four.

Mr. Karl Langenbeck:—I think Mr. Stover's point is of interest to those connected with the clay industry proper. This particular application would not be in any place where it would come in contact with fuel, or any active chemical agent, as at the kiln mouth, but merely as slabs for lining the muffles. The question would be whether the hydrated silicate which forms the bond in sand-lime brick would pass into an igneous silicate bond, at cone eight or ten? What would be the influence of repeated heatings and coolings of such a product? That is the movement which racks such structure.

Mr. Peppel:—It would simply be the same as that of a silica fire brick, because when you bring sand-lime brick up to the point of vitrification, you simply make a silica brick of it.

Mr. Langenbeck:—The movement of silica under sufficient degree of heat is that of expansion, and possibly afterwards a slight contraction again. The question is, will this movement in this material probably assist in maintaining the structure, and make it preferable to the fire clay product, which we know contracts and in time racks? Are there any experiments with which you are familiar, or do you recollect about the behavior of the *Dinas* brick under heat?

Mr. Peppel:—I have no evidence to show, but I am of

the opinion that you have outlined the case correctly. The material is sufficiently porous to take up slight expansion in the very small amount of binding material; when the structure would reach the end of this ability to take care of expansion, the binding material would be in condition to flow. The expansion of this material at a little below cone thirty is reported from a German source to be about two per cent.

Mr. Langenbeck:—Then even with five per cent. of lime, it does not melt at cone thirty.

Mr. Yates:—I will inquire of Mr. Langenbeck if he considers five per cent. of lime added to free silica, sufficient to bring it to fusion at kiln heat?

Mr. Langenbeck:—No, I think not; but I am trying to get at just the point you are trying to get at—what would be the extreme limit of temperature which these brick would stand. We now hear that they did not fuse at cone thirty, and it is likely cone thirty-three before actual fusion takes place. This will give some rough idea of what we might expect under ordinary pottery conditions, at cone eight or ten. If, *without breaking down the structure*, the hydrated silicate of lime bond had passed into a igneous silicate of lime bond, and if actual fusion does not take place until you reach cone thirty or thirty-three, it looks as if it were an available fire-resisting material for certain parts of our kilns and muffles, with the promise that the movement which it undergoes is a less destructive movement than that of fire clay, with its continual shrinking under continued heat.

Mr. Yates:—I would further inquire of Mr. Langenbeck if he considers it will be a safe material to use, taking into consideration the expansion of silica? Would not the spalling and chipping of the silica brick be more destructive than the shrinking of the clay brick?

Mr. Langenbeck:—I fancy it would, and for that reason I put the question about expansion. Mr. Peppel has given his opinion, that while the total expansion of the silica is very small, as shown by tests, at such temperatures as interest us, say cone eight to ten, the expansive movement would

be taken up internally in the brick themselves and would not exert a total swelling force to amount to anything.

Professor Orton:—I think the discussion has taken a line which is probably most dangerous for sand-lime brick. I don't believe anybody has seriously put forth sand-lime brick as a suitable material to replace genuine *silica* brick. I do not see where any economy would result in using them. Where sand-lime brick of the *silica-brick* type are manufactured, they burn them at exceedingly high temperatures. They do not depend on any sintering bond, gotten at low heats. I sell cones to manufacturers of silica brick of that type, and theirs is almost the only demand I have for cones over number twenty. I had to specially make cones pretty near to the top of the series for these people.

Now, in brick made by this hydraulic process, with say twice or three times as much lime as is used in the normal silica-brick, why should we expect to destroy the hydrous silicate produced in hardening kettle, and replace it with an igneous silicate, without going up to high temperatures? This brick is put forth principally as a building material, and Mr. Peppel has shown us here today, the different qualities of sand and their effects, what impurities may safely be present, and how much. He has shown what kind of lime is best and the amounts which may be used; he has shown what pressures are necessary, both in the die and the steam cylinder. From my point of view, he has covered about all the phases of manufacture. Last year he showed us about how big an investment was necessary to get started. But, all this bears upon the production of tolerably cheap brick for building purposes. The question that seems to me most interesting, is where this particular kind of brick manufacture is most likely to take root to advantage.

Mr. Peppel:—While it would probably be possible to compete with clay brick, in the heart of the clay country, provided the sand and lime were at hand, I hardly think that in the next ten years we will see much encroachment upon clay territory by the sand-lime brick. The fuel consumption is very small, so it is desirable to try it first in

a locality where fuel is expensive. For that reason, those locations should be first selected where sand is abundant and fuel expensive. It certainly has the best chance there, not only to compete with clay bricks, but to get large prices for the product.

THE FORMATION TEMPERATURES OF CERTAIN SLAG SILICATES.

BY

PROFESSOR H. O. HOFMAN, PH. D.

The Chair: Gentlemen, we are to be favored this evening with a lecture upon a new phase of our field. At the earnest solicitations of the secretary, Dr. H. O. Hofman, Professor of Metallurgy in the Massachusetts Institute of Technology, has kindly consented to lay before us the results of his celebrated investigations on the "The Formation Temperatures of Certain Slag Silicates", and will show us the apparatus and methods he used in prosecuting his researches. I have the honor to introduce Dr. Hofman. (Applause.)

NOTE BY THE SECRETARY.

Professor Hofman, after making a few graceful introductory remarks, entered at once into the consideration of his subject. The subject matter and results have already been published in excellent form in the Transactions of the American Institute of Mining Engineers, Vol. 29, page 682.

The expense precludes their republication here, but those interested may procure reprints of the article by corresponding with Dr. Hofman or Dr. R. W. Raymond, Secretary of the American Institute of Mining Engineers, New York.

DISCUSSION.

Mr. Karl Langenbeck: Dr. Hofman, you say that all of your observations relate merely to formation temperatures and not at all to fluidity, and you mention about the difficulty of finding out anything about that factor. When we potters want to compare the fluidity of two glazes, we simply make cones of them and submit them to heat in a Seger furnace on a slab tilted at an angle of forty-five degrees. We see when they run down, which is most fusible. What objection would there be to employing that method with slags?

Dr. Hofman: The difficulty with slags in which iron forms a part of the base, is that some of the iron exposed to the air may be oxidized, and that changes its fusibility.

Some of the irregularities shown in my charts are due to changes from ferrous oxides to ferric.

Professor H. A. Wheeler: Were all these temperature observations on these diagrams made by means of Seger cones, Dr. Hofman, or with the Le Chatelier pyrometer?

Dr. Hofman: All were made with Seger cones. When I first began these experiments, I tried to use the Le Chatelier pyrometer. I took the slag mixture, and first put it in a platinum crucible and heated it. I found the platinum crucible converted into a carbide, and not being rich, we could not afford it. We tried another experiment, taking the Le Chatelier junction, and casing it around with some of the mixture, heating it carefully at first, increasing the temperature, and watching the increase of temperature. The point of retardation, or momentarily constant temperature shows the point when the silicate is formed. That formed a nice method of determining temperature, but the platinum wires would become brittle and break, and we had to give up the Le Chatelier pyrometer.

Professor Wheeler: You have not verified any of the Seger cones by means of Le Chatelier pyrometer?

Dr. Hofman: No. I accepted Cremer and Hecht for that. They have verified them. I think one is the melting point of silver, and the other that of an alloy of gold and platinum somewhere higher up. I forget just the temperatures, but there are one or two fixed points, I think, and between these two fixed points, a certain number of Seger cones have been interpolated. One melts after the other; and it is assumed that temperature intervals are equal. So, as I said before, in using Seger cones, if you want to be scientifically correct, you must leave out degrees centigrade, and give your results as occurring at Seger cone so and so.. Degrees centigrade are only used to bring the results into comparison with pyrometers.

Professor Wheeler: To what do you attribute the irregularity of the curves?

Dr. Hofman: Do you mean in curve number four, singulo-silicate, where it makes a sudden jog? I cannot give any reason for that. Of course when this jog appeared, we

rather began to doubt, ourselves, whether that point was the real one, and I cannot just say how many experiments were made to settle it. Why it should go down here, and up there (indicating on chart), I do not know, but it is a fact that it does.

Professor Wheeler: Don't you think it is due to the irregular way in which Seger cones act?

Dr. Hofman: I do not think so, quite. I think any irregularity is not due so much to Seger cones, as to the fact that it is so difficult to keep ferrous oxide in a ferrous state. Take a mechanical mixture and form it into a chemical compound—if you get within fifteen or twenty degrees centigrade of the actual fusing temperature, I think you are working very closely.

Another point to be considered, is that the formation change of a mechanical mixture into a chemical compound is not simply a function of temperature. It is also a function of time. You will find somewhere of record (I think in Cremer), where a porcelain factory in Berlin, baked in two furnaces the same mixture of porcelain (I think I am quoting it correctly, tho it is a long time since I read the paper) and controlled the temperature with a Le Chatelier pyrometer. In one case, they fired in the normal way, with the regular rise of temperature until they reached the maximum, and then decreased the temperature in the normal way. They got up to a certain temperature and did it in a given time. The next time, for some reason or other the fireman did not fire regularly, and instead of having to fire to the same point to obtain the correct baking of the porcelain, they had to fire higher, thus showing on a large scale, in regular work, that this change of a mechanical mixture into a chemical compound is not simply a function of temperature, but time has something to do with it, and the method of firing, and the form of furnace has something to do with it.

Mr. Langenbeck: I think we are very much indebted to you for this expression of your views. We have waged quite a battle with Professor Wheeler on this point for several years, and he has tried to draw from you evidence to support his side. (Much laughter.)

Dr. Hofman: I do a great deal of work with the Le Chatelier pyrometer, in the melting point of alloys, etc. As soon as it is determined that a thing takes place at a definite temperature, there is nothing like the pyrometer; but the fusion of silicate takes place in a range of temperature.

Professor Wheeler: Dr. Hofman has not only come to my side, but there is the proof in black and white (pointing to the charts on the wall) of the trouble from the use of Seger cones.

Dr. Hofman: I disclaim that.

Mr. Langenbeck: I will ask Dr. Hofman whether there has been any work on this line, running more into the acid silicates? Our interests as ceramists, are mainly with acid silicates—little or nothing with basic silicates.

Dr. Hofman: As far as I know, no other work of this kind has been done, in which iron forms one of the bases. As I said in the beginning, Professor Akerman, of the Royal School of Mines in Sweden, has done splendid work on the blast furnace slags or the silicates of alumina and lime. I cannot recall off-hand how far he carried it, but his paper has been translated into several languages—we have it down stairs.

Mr. Langenbeck: Another question. Of course, under the line of your work in formation of slags attendant upon the reduction of metals, it is only the form of ferrous silicates that interests you. At the same time, I would like to ask if you have done any other work which would probably give some general information on the presence of ferric, as well as ferrous compounds? It seems to me that in testing the refractoriness of clay containing more or less iron, that it would be important in stating comparisons against Seger cones to mention whether under oxidizing or reducing conditions: otherwise, the comparisons would be of comparatively little value. Isn't that the case, or don't you think that it makes much difference as to whether the iron is ferrous or ferric?

Dr. Hofman: The ordinary fireclays contains such comparatively small amounts, I doubt whether it has a decided effect. I do not think it would have a decided effect. In

the laboratory work here in fire-clays, we use the DeVille furnace, and of course the clay is surrounded entirely with gas-carbon, and I do not think there can be any strong oxidizing reactions taking place. It may be a different thing when put into an open furnace and tested there.

Professor Edward Orton, Jr: It seems to me that Dr. Hofman has done this society a great service in presenting this phase of silicate formation to our attention. I have persistently taken the attitude during the formative years of the society, that we represent the whole field of silicate industry. We have brought into the organization not only the potter and the brickmaker, but we have also ensnared the cementmaker, and now for the first time, we have had an address upon a phase of silicate formation, the point where our sphere touches that of the metallurgist. Dr. Hofman's contribution should be especially welcomed on that account.

I have listened with exceeding pleasure to Dr. Hofman's address tonight. My own experience in making fusible glasses, concerning which I read a paper a year ago, confirms the general character of his curves but at a very much higher stage of silicate formation. I was dealing with acid glasses, while he was dealing with basic ferro-calcic silicates. His plane of operation is below mine, but if I could plat the two curves, they would be substantially parallel.

Dr. Hofman: Professor Akerman's curve is similar to that one—so there are three.

Professor Orton: I made a series of glasses last year and found that with what in your method of expression would be a five-fold silicate, I had ordinary window glass. By reducing its acidity to four, it reaches a slightly more fusible stage. By reducing to three, we get approximately about the same fusibility as four-silicate; reducing further the curve began to go upward again as yours there does, tho your series and mine were separated by a wide difference of temperature.

Where the curve makes zigzags like that after repeated trials, I would like to ask whether you think it fair to say it is due to the mass of the material entering into the formation of definite silicates, at the low point of the curve, and to the

ratios being such that there is a good deal of material left over in other cases, which does not enter into combination, and thus causes the high points of the curve?

Dr. Hofman: This is a question of solution. We now generally look at slags as solutions of one silicate in another; so if a slag is composed of one silicate dissolved in another, we obtain low fusion temperatures; and when we get an excess of either acid or base, it does not so readily dissolve and hence causes irregularities. So where we have these irregularities in the curve, we think the lowest points may be reached by the joint solution of two silicates, and the higher points may be the only mixture of the two silicates.

Professor Orton: These curves of Professor Hofman's slags, seems to me to have a definite bearing on the subject discussed yesterday morning, viz., the formation of matt glazes. When we fuse up a mixture for making a matt glaze, it is not certain we shall produce a compound which will crystallize well. But if the theory that the formation of matt glazes is due to compounds which readily crystallize be true, then it seems likely that there are certain points between acidity and basicity in the composition of the glaze exceptionally favorable for the production of this crystallizing silicate. Those points may be indicated by such dips or elevations as those in these curves. Other points between the same two extremes might not give a good matt. Those who produce matt glazes in a commercial way, state that the range of composition and temperature within which they may be produced satisfactorily is exceedingly small. That is, you must get scientifically or empirically at one of the points where some particular mineral in solution in the glaze will crystallize out easily and rapidly and with such a size of crystal as will give the soft silky texture and sheen which the good matt shows. Otherwise, you may get a glaze which is matt in one sense, *i. e.*, not bright nor glassy, but which is simply immature, or dull from the segregation of matter either insoluble in the other silicates of the glaze, or which

is cast out from solution, without crystallizing, as amorphous separations.

On motion, the president extended the thanks of the society to Dr. Hofman for the presentation of a lecture in which the members had found so much interest and which was so pregnant with lines of profitable thought and investigation.

"THE STRUCTURE OF SILICATE MIXTURES."*

DISCUSSION BY A. V. BLEININGER, B. SC.

The note on "The Structure of Silicate Mixtures," by H. E. Ashley, touches on a point which is of the greatest interest to the ceramist. We all realize, I believe, the deficiencies of purely chemical research and it must be our aim to supplement the gaps left by chemical work, with physical methods along lines similar to those followed by the metallurgist and using the means offered by physical chemistry and mineralogy. It will, perhaps, not be out of place to look over some of the scientific equipment now available for the attacks of the problem of the constitution of the silicates. We have at our disposal the following physico-chemical methods of investigation:

1. The determination of melting points.
2. Microscopic and mineralogical examination.
3. Thermo-reactions of neutralization, solution, hydration and determination of specific heats.
4. Determination of water of hydration.
5. Velocity of reaction.
6. Optical properties.
7. Solving power of liquid silicates.
8. Partial solution of silicates by re-agents.
9. Determination of density, elasticity, tensile strength, crushing strength and hardness.
10. Co-efficient of expansion.
11. Electrical properties.

Melting point determinations of silicates, especially of slags, have been made in great number and some valuable information has been drawn from them as is shown by the work of Akerman, Hofman,[1] H. E. Ashley[2] and Jüptner.[3]

This work is capable of great extension, especially since

*Original paper published in Trans-Am. Ceramic Society, Vol. IV.

[1] Transactions American Institute of Mining Engineers, Vol. 29, p 662.

[2] Transactions American Institute of Mining Engineers, Vol. 31, p 854.

[3] Siderology, The Science of Iron. Hanns Freiherr Von Juptner, translated by Charles Salter, 1902.

electric furnaces are becoming a recognized part of mineral laboratories, thus eliminating the factor introduced by the fire gases. The work done by several members of this society (Prof. Orton, Mr. Purdy) illustrates this point very clearly.

Microscopic examination also offers a vast and exceedingly fertile field, which should be cultivated much more than it is. It is and must be intimately associated with mineralogy. Already in the examination of raw materials its great importance is shown. Many problems can only be solved with the aid of the microscope. The character of many clays can be fixed definitely only by means of it. In fact, the application of the microscope has advanced to such a stage that it has been suggested by an eminent authority, Prof. Rosenbusch, as a means of analyzing rocks quantitatively in thin sections, using glass slides provided with fine cross-lines which divides the surface into squares. The area occupied by each mineral is estimated by means of a simple calculation, and in this way the rock is analyzed quantatively for its mineral constituents.

The qualitative examination of crystals produced from silicate mixtures is well illustrated in a paper by W. Jackson and E. M. Rich.[1] in which is shown a number of microphotographs of crystals produced on the surface of different glasses by etching with dilute hydrofluoric acid, which bring about partial solution. It seems a pity that these investigators did not go further into this study of annealing the glasses for longer or shorter periods, thus producing crystals in greater numbers and variety. Take for instance, the hard glass tube of a carbon combustion furnace; after being heated for some time, the glass gradually de-vitrifies and finally assumes a porcelain-like appearance.

It does not seem probable to the writer that mere condensation of vapors on glass surfaces will give a satisfactory picture of the micro-structure, since too many surface phenomena are liable to interfere, and since the surface itself does not represent the structure of the main mass.

Several years ago, I made a thin section of a paving

[1] Journal Society Chemical Industry, Vol. XX, p 555, 1901.

brick specimen collected by Prof. Orton, which showed in a remarkable manner three zones of vitrification, 1st, good, sound vitrification with red color; 2nd, over-vitrification, accompanied with dark color, and 3rd, the beginning of vesicular vitrification. Examined under the microscope, the zone of over-vitrification was distinctly indicated by the formation of needle-like crystals, which however I did not succeed in identifying.

Another use of the crystallographic examination is represented by the work of Dr. Schott, who in the eighties, examined fused solutions under the microscope, allowing them to crystalize out and observing the crystalization in its very genesis. By means of a gas flame and suitable shields he kept the solution at a red heat and thus was able to regulate the cooling and heating up at will; he was thus able to redissolve crystals noting their relative melting points. In this way he could study igneous reactions which often differ radically from wet reactions involving the same substances. Now when we have at our disposal, electric furnaces of all kinds, it would seem a very easy matter to maintain any desired temperature.

The determination of specific heats should, in the nature of the case, give valuable constants, and could be applied without great experimental difficulties. These heats of reaction and neutralization as applied to cements, and soluble silicates in general, have been used by several experimentors and always produce valuable comparative constants. An example of this is the testing of lime by means of the calorimeter in which the heat evolved on hydration or by solution offers a rapid commercial method.

The determination of the water of hydration is applicable to clays as well as to cements; in the former it is a well known fact that this furnishes an index of the purity of clay. In testing trass for cement-making, the content of combined water is made a point of commercial importance. In cements, the hydration values proved valuable supplementary evidence.

The velocity of reaction is applicable to clays and glazes only in a limited degree, in regard to the rapidity of

fusion. For cements, the velocity of the hydration reaction is of great importance, and divides cements into large characteristic classes.

The optical properties of glass, the indices of refraction and dispersion have been examined quite thoroughly by Schott, and his associates at Jena, with reference to the ratios of different bases in the composition, but I do not believe that the acidity and the effects of systematic additions of alumina as applied in glazes have been studied from this standpoint. It might be possible that the application of the Lorenz formula could throw light on questions of constitution.

Polarization, of course, properly comes under the head of mineralogical examination, where it is constantly used. Colorimetric examinations (see Ostwald Physiko-Chemische Messungen, page 246) should be applied extensively for color reactions and phenomena in glasses and glazes.

The solving power of silicates in igneous fusion for various reagents could be made a valuable feature in ceramic work, since chemistry as a whole is becoming more and more a question of solubility. For instance, the solving power of definite glasses for certain bases, like lime, or acids, like tin oxide, at certain temperatures must be a function of the acid-base ratio.

Partial solution has already been illustrated by citing the work of Jackson and Rich, who used hydrofluoric acid to etch glass. A large number of liquids are available for this purpose.

The determination of the physical properties hardness, tensile strength, density, elasticity, crushing strength, etc., considers properties whose importance for the examination of certain bodies depends of course on the particular substances under discussion and the purpose of the examination. Messrs. Jackson and Rich made use of the difference in density in endeavoring to separate the different constituents of the extremely fine ground glass, assuming that the glass is a mixture of various silicates more or less homogeneous, according to the views of Benrath. This they did by means of the Schoene funnel and the Thoulet solution, a suspension

method using a methyl iodide solution of different density, whereby some constituents would float and others sink. The probability is that the work was not successful, and it would perhaps have been better if the glass had been annealed to produce crystallization. In this connection, it might be mentioned that Daubree[1] already has shown that ordinary glass is decomposed by superheated steam into quartz, wollastonite and alkaline silicates.

An interesting study suggested would be the investigation of the density of porcelain at various stages of burning.

The expansion of solids by heat, the co-efficient of expansion of glass as well as other ceramic bodies, and the rate of expansion for rising or falling temperatures promises to give valuable constants, the difficulty being, however, the extremely delicate measurements and the great amount of time consumed in this work.

The least promising physical data are perhaps based on electrical phenomena, electrical resistance and the study of magnetic fields produced by electro-magnets. A number of years ago the writer did some work in separating grains of iron pyrites from dried clay, by means of an intense magnetic field. Though the experiments were not completed, they were successful as far as they went.

This little summary is intended to indicate the present scope of the available physical-chemical methods, for one must remember that theoretical results are not proven by one set of constants, but it requires several coincident experimental factors. At the same time, we should realize the limitations of the methods, as well as of the material to be examined. For instance, microscopic examinations of soft burnt clay will reveal but little and should not be expected to furnish important data.

[1] Ann. des Mines, Series V. No. 12, 1857.

THE WORK OF THE CERAMIC ENGINEER IN THE BRICK-MAKING INDUSTRY.

BY

WILLARD D. RICHARDSON, A. M., COLUMBUS, O.

There is constantly increasing evidence that manufacturing businesses are becoming more and more dependent for success upon engineering. Engineering is applied science, and all business, especially all manufacturing business, is reducible to a scientific system, and its operations to certain definite scientific laws. It is the work of the engineer to ascertain these laws and apply them successfully to every special case under his charge. The commercial value of an engineering education is becoming more generally appreciated by business men. The growing popularity of engineering schools and the constant extension of engineering courses is a marked feature of our recent educational development and is certain to be far reaching in its influence upon the industrial progress of America.

Clay-working, one of the oldest and most important of the practical arts, is one of the latest to receive the benefit of scientific instruction. Legislators have only recently been brought to realize the industrial importance of the ceramic arts and to make appropriations for ceramic departments at some of our state universities. The ceramic engineer has in this country only just come upon the field, and his labors so far have been given chiefly to pottery and the more artistic forms of clay-working. To arouse greater interest in the need of the services of the ceramic engineer in the brick-making industry is the object of this short paper before our society. In the brick-making industry I include for this purpose not only all forms of building brick, paving brick and fire brick, but also drain tile, sewer pipe, roofing tile and terra cotta.

The large loss of money in the brick business is notorious. There is hardly a community that has not experienced

some failure or waste of capital in brick-making enterprises. In consequence even legitimate projects in this line are looked upon with suspicion and often cannot secure the confidence of investors, and thus a barrier is placed to the healthy development of this industry. Of course, no means can be found that will entirely prevent unsound ventures in any business, but a large majority of the failures in the brick business can and should be prevented through the agency of the consulting ceramic engineer.

Brick plants are often established without good reason, without such investigation as it would seem that ordinary business sagacity would require. They are perhaps located where the clays are too limited in quantity and too varying in quality, or practically inaccessible, the cost of mining being excessive, or not suitable for the product desired; or where the facilities for manufacture are unfavorable or the market inadequate or remote. Or the yard may have been improperly planned and equipped, and employing a process for which the clays are not adapted or against which there is a prejudice in the market. Or the machinery, dryers and kilns are not such as should have been selected for the most satisfactory and economical results. In fact the whole establishment shows a lack of technical knowlege that should have been supplied by the ceramic engineer. Had he been consulted, perhaps he might have advised after a preliminary investigation, the abandonment of the undertaking on the ground that the conditions were not favorable for profitable investment. Or, after exploiting and testing the clays and carefully examining all the conditions, his favorable report would have secured him a commission to make complete plans and specifications for the construction of a plant; and so well would he have fulfilled this commission that the success of the enterprise would have been assured. His fees for this engineering work would amount to a very small percentage of the cost of the plant, and would not only not have increased the cost of construction, but his services in preparing definite plans and specifications upon which competitive bids could be secured, would have reduced the total cost by more than the amount of his fees.

The ceramic engineer will frequently be called upon to correct faults of manufacture, to establish more economical methods and to effect such a reorganization as will place the plant upon a paying basis. Sometimes it is true he may be called in too late when the money is all gone, the property mortgaged and no means can be found to apply the necessary remedy. Had he been called in a year or two earlier, he might have prevented the loss of several thousands of dollars, but at that time perhaps the projectors were so confident of success, and of the sufficiency of their own knowledge, that they would have been unwilling to pay a few hundred dollars for the services of an engineer.

A competent consulting engineer should be a man who has not only pursued and completed a scientific course of study especially applied to the clay-working industries, but who has had also considerable practical experience in manufacturing clay products. He should be able to exploit and test clays, and advise the best methods of working, to design factories, dryers and kilns, and to specify the proper machinery. He should also be able to remodel and reorganize old plants and wrest success from failure wherever possible.

Under present conditions it is easier to outline the work of the ceramic engineer than it is to assure him of such employment. However beneficial might be the services of the ceramic engineer to the brick manufacturer, it is not to be expected that the opportunity for such service will be readily afforded. Brick manufacturers in this country, and especially those about to engage in new enterprises, are not yet educated up to the importance from a practical business standpoint of employing a consulting engineer to investigate their propositions, or faults of manufacture, or to specify improvements in their methods. This has come about only slowly in other industries and will doubtless be of slow development in ours. So that while there is a splendid field of labor for the consulting ceramic engineer in the brick-making industry, he cannot as yet be assured of remunerative employment. And the ceramic engineer I suppose, like all other consulting engineers, must needs have a code of ethics that would prevent any active solicitation for clients. At least

such solicitation would be undignified and distasteful to a man of education and refinement, however much he might thereby extend his usefulness. But at any rate, it must be in his code not to solicit nor accept commissions from anyone on account of labor or material furnished under his specifications or directions. This is good morals for others, as well as for engineers.

Graduates of ceramic schools will not be likely to assume at once the profession of consulting engineer, and should not be encouraged to do so until they have had several years experience in practical operations. But it is from this class of men that engineers for the brick-making industry are to come. This has been the result of technical education in Germany. Ceramic schools established there long ago, have produced a class of engineers that have devoted themselves to brick-making, and by their labors this industry has made such progress in the solution of technical problems, has been brought under such control, has been made commercially so sound and profitable, as to incite the emulation of all progressive brick-makers everywhere. With them, brick-making is no longer an empirical art but an established science. The construction of brick work is not with them a haphazard matter, a "rule-of-thumb" procedure, but a carefully planned and accurately calculated undertaking. All operations of manufacture are conducted according to established scienctific principles. The result is a product not only adapted for the purpose for which it is to be used, but one that has been economically made, thus insuring a profitable investment.

Such will be the results in this country of the educational movement now in progress. Unsound brick-making projects, improper equipment and wasteful methods will come under the control of the ceramic engineer. We now undoubtedly lead the world in the efficiency of our brick-making machinery, but in accurate technical knowledge of clays, and the best methods of treating them for special purposes, in a scientific system of drying and burning, in fact in all the chemical processes involved in the manufacture of clay products we have much to learn, and there is only one

way to learn it, and that is by the training of our young men in technical schools, giving them a thorough course in ceramic engineering. The rest will follow. Such of these men as enter the brick-making industry, will gradually bring about a more scientific system, that will give more positive and certain results, a better product at less cost, a business with less risk.

The time will come when few brick plants of any pretensions will be built without the advice and assistance of the ceramic engineer. He will be engaged at the start to exploit the clay deposits, make analysis of the clays and practical demonstration of their value. He will also examine all the conditions that have a practical bearing upon the proposed establishment. His report upon all these points will determine the action to be taken. If he reports favorably, and recommends the project, the next thing is to prepare plans and specifications for the complete plant, having in view the nature of the material, the kind of product desired, and the amount of capital to be invested. This will call for his best skill and judgment, as upon it depends largely the success of the whole business. He will supervise the construction and starting of the plant, and give instruction in all the operations of making, drying and burning. He will act as consulting engineer in investigating and correcting any faults or difficulties of manufacture that may arise. This in brief is the work of the ceramic engineer in the brick-making industry; to give to it his time, his energies and a mind specially trained for the study and solution of ceramic problems, and thoroughly alive to the importance and to the possibilities of the brick-maker's art. Through his agency alone, the first quarter of the 20th century will witness such advancement in the art of brick-making, such improvement of all conditions of manufacture, such an elevation of the financial standing and social status of the brick-maker as will give to brick-making a foremost place among the world's industries.

NOTES ON THE CONSTITUTION OF CHROME-TIN PINK.

DISCUSSION BY OWEN CARTER.*

POOLE, ENGLAND.

Referring to Mr. Hull's interesting paper in Vol. IV of the Transactions, on the "Constitution of Chromium-Tin Pink", I would beg to submit the following notes. I fear that my remarks on this subject will not be as suitable as I could wish, owing to the fact that we English potters fire our Majolica glazes, colored glazes or enamels, as they are variously called, at a lower heat than our American friends appear to do. Again, I am not quite sure if the color Mr. Hull was working upon was of the same kind as the piece I enclose,[1] and propose speaking of, which in England is known as "Sang de Boeuf," a very popular color and considerably in demand. There are of course innumerable chromium-tin shades and colors, but this deep, rich tint was that with which we had most trouble, and may, I think, be taken as more or less representative of the whole group, even though the maturing point of the glazes be different.

I understand that Mr. Hull's experiments were conducted with the object of gaining information:

Firstly. As to the most favorable composition of the pink frit, and

Secondly. What type of low fire glaze developes the color to the best advantage.

With regard to the first question I do not think it much matters what formula one takes, provided it is one of the many that will make a decent strong pink, crimson, or

*Read by title at Boston meeting. Further discussion specially invited.

[1]Note by the Secretary. Mr. Carter sent with his paper several small shards glazed very handsomely with one of the finest chrome-tin red glazes it has ever been his fortune to see.

maroon underglaze color. What the exact tint may be when used as an underglaze color can hardly matter in the least, as when it comes to be mixed with the glaze, its proportions are entirely upset. We use the following for the pink frit:

26 parts oxide of tin.
14 parts whiting.
1 part bichromate of potash.

thoroughly mixed and calcined in the hardest place in our vitreous kiln.

The second question is, in my opinion, the all important one. I am ashamed to think now, of the many trials made and the time they took, not to speak of the trade lost, before we succeeded, and all because we did not realize the importance of this second question of Mr. Hull's, Some times (probably because the heat was by chance just suitable) we would get fair results, and feel sure we must be on the right track, but on trying again the same recipe, the color would be either not developed or gone entirely, because probably the exact heat had not been hit upon, and such an exactness of temperature was in practice impossible. This sort of thing is far worse than downright failure, for it keeps one hanging on and wasting time. At last, it dawned upon us that the trouble might be in the glaze, so we took two frits rich in lime and silica, one alkaline and the other a soft lead frit,

Alkaline Frit.		Soft Lead Frit.	
Borax	150	Litharge	240
Flint	75	Cornwall stone	90
China clay	50	Flint	50
Whiting	50	Whiting	10

mixing them with the pink underglaze color thus:

	A	B	C	D	E	F	G
Soft lead frit	30	25	20	15	10	5	..
Alkaline frit	..	5	10	15	20	25	30
Pink underglaze	2	2	2	2	2	2	2

These were fired at cone 09, the results being as follows:

A. Rich mellow red, but rather too brown, hardly life enough, but still a beautiful color.
B. A little more red than A.
C. } D. } Beautiful bright crimsons.
E. Good crimson but decided tint of purple.
F. A stronger tint of undesirable purple.
G. Disagreeable bluish purple color.

Some delay was now occasioned by "A" being the most mellow and artistic shade, and, thinking a little more fire would develope a brighter color, we made more experiments on this line. But it was soon discovered that it required too exact firing for practical purposes and that when "C" and "D" would be still bright and good, "A" would be spoilt entirely. Then we tried a harder lead frit for "A," still using no alkaline frit, the result, though better, was still unsatisfactory in the same way as before. It was therefore decided to try and make "C" and "D" more mellow with chromate of lead, which proved successful. The following frit was then constructed which will be seen to be 100 parts of the soft lead frit; 60 parts of the alkaline frit:

Borax	90	Frit at 09 cone, grind and dip without admixture of any raw material. We wash neither the under-glaze color nor the frit, but it might be better if this were done.
Flint	95	
China clay	30	
Whiting	40	
Litharge	240	
Cornwall stone	90	
Maroon underglaze	29	
Chromate of lead	6	
	620	

Small charges of this color were fritted in flinted saggers in front of enamel kilns at 09, and thorough trials were made in different kilns. It was found to stand slight variations of heat better than we had dared to hope; the color also, seemed to be the popular shade. No further alteration in the recipe was then made, and the formula now stands as above, some tons of frit having since been made from it. This method of calcining in saggers in the fronts of enamel kilns was soon found inadequate, as the color was selling too well to keep going in this way. We therefore

thought we would try fritting it in the crucible kiln, the crucibles being fed at the top, and the molten frit running out into water through a hole in the bottom of the crucible. The type I mean is of course well known to you all. What was my horror when walking round the works a day or so after this was decided on, to see the crucible kiln in full swing, and a bright grass-green frit in the casks, which I was told was "Sang de Boeuf." Needless to say, I stopped the fritting at once, had some frit ground, and trials of it got in the kilns with as little delay as possible. I fully expected them to come out a bright green like the frit, but to my surprise and delight, they were, if anything, a brighter and better red than tiles dipped from frit which had been calcined in saggers.

Thus ended our troubles, up to the present, with this particular color, and if the narration of our fruitless labor and the way success was attained, will help our fellow potters, we shall be more than glad. The recipe is probably capable of improvement, but in a pottery there are so many things to do that one can only spare time for the most urgent.

DISCUSSION BY OTTO HENSEL,

NEW LEXINGTON, OHIO.

An attractive red glaze, with a tinge of yellow, or at least without trace of the ordinary purplish color being very desirable in terra cotta effects, I made some experiments in trying to obtain it by the use of tin, chrome, and lime.

The body was ferruginous and quite high in alkaline earths. The only kilns available for fritting the color and firing the glaze were the ordinary down-draft, and the Stewart kiln, a modification of the down-draft, wherein the fire-gases pass through flues under the solid floor, up through bags or flash-walls on the opposite sides respsctively, then down through the setting and out through small flues near the bottom in the walls of the other two sides. Oil was used for fuel.

The most remarkable oxidizing conditions appeared to prevail on the floor and for several feet up in these solid-bottom kilns. Almost any haphazard mixture of the ingredients required for pink and the allied reds would give beautful clear glazes of a desirable color. I may add that the glaze was put on the raw body, and had to undergo the slow firing required for green ware. The above circumstance lured me into an unproductive attempt to use the tin-chrome formulae under down-draft firing conditions. The solid-bottom Stewart kilns were altered to ordinary down-draft in the course of my work.

At first, color batches were fritted on the floor of the solid-bottom kilns, where a temperature probably equal to Cones 04 and 03 was reach. Pulverizing and a second firitting was found to produce much more stability and strength of color in the glaze. An intermediate washing also improved the color to some extent.

The first series of colors produced very fine, almost direct reds, under oxidizing conditions, but were badly reduced otherwise. The best of this series were:

$$1 \quad \left.\begin{matrix} 0.03\ K_2O \\ 0.07\ Na_2O \\ 0.40\ CaO \end{matrix}\right\} \left.\begin{matrix} 0.03\ Al_2O_3 \\ 0.002\ CrO_3 \end{matrix}\right\} \begin{matrix} 1.00\ SiO_2 \\ 0.14\ B_2O_3 \end{matrix} + 0.08\ SnO_2$$

$$2 \quad \left.\begin{matrix} 0.03\ K_2O \\ 0.05\ Na_2O \\ 0.42\ CaO \end{matrix}\right\} \left.\begin{matrix} 0.03\ Al_2O_3 \\ 0.003\ CrO_3 \end{matrix}\right\} \begin{matrix} 0.70\ SiO_2 \\ 0.10\ B_2O_3 \end{matrix} + 0.08\ SnO_2$$

In the amounts given here, they were added in a glaze in which the RO including these given, footed up 1.00. The first color gave a fine deep red, lead being 0.44 equivalents in a medium hard glaze. Even when reduced, the reduction was not complete. The second color with somewhat less spar and 0.48 equivalent lead gave a beautiful yellowish red, a sort of crimson, and proved rather stable under reducing conditions.

The formulae given by Seger in his article on underglaze colors, such as,

$$3 \quad \left.\begin{matrix} 0.032\ Na_2O \\ 0.750\ CaO \end{matrix}\right\} \quad 0.03\ CrO_3 \quad \left\{\begin{matrix} 0.900\ SiO_2 \\ 0.064\ B_2O_3 \end{matrix}\right. + 1.00\ SnO_2$$

$$4 \quad \left.\begin{matrix} 0.023\ Na_2O \\ 0.840\ CaO \end{matrix}\right\} \quad 0.01\ CrO_3 \quad \left\{\begin{matrix} 1.780\ SiO_2 \\ 0.047\ B_2O_3 \end{matrix}\right. + 1.00\ SnO_2$$

$$5 \quad \left.\begin{matrix} 0.033\ Na_2O \\ 0.750\ CaO \end{matrix}\right\} \quad 0.023\ CrO_3 \quad \left\{\begin{matrix} 1.410\ SiO_2 \\ 0.066\ B_2O_3 \end{matrix}\right. + 1.00\ SnO_2$$

were next tried.

These colors gave their finest results in glazes in which the RO was composed as follows:

Lead—About 0.50 equivalents, although it went to 0.63 in one fine glaze, in which one-eighth of color number 3 was mixed so as to have the following total composition:

$$\left.\begin{array}{l}0.630\ PbO\\0.314\ CaO\\0.054\ KNaO\end{array}\right\}\ \left.\begin{array}{l}0.15\ Al_2O_3\\0.004\ CrO_3\end{array}\right\}\ \begin{array}{l}1.792\ SiO_2\\0.108\ B_2O_3\end{array}\ +0.125\ SnO_2$$

This glaze showed some bubbling, due most likely to low alumina. The red developed well in this glaze, though it was very susceptible to reduction. Aside from the color and some quicklime, all the ingredients used here were raw.

Soda—Inclusive of what little spar I could use, 0.25 equivalents, or less. High soda was conducive to stability, and produced more yellowish effects as against the purplish of high lime.

Potash—As carbonate or nitrate, gave a more pleasing color than soda, but in the few trials that I made, I found the glaze rather more susceptible to reduction.

Lime—Had to be as high as 0.25 to 0.36 equivalents.

Zinc—Was found to be of no service.

Boric Acid—0.10 equivalents, although a very beautiful deep purple was obtained with 0.16 equivalents.

The next experiment tried was a series of colors to see what good would come of the incorporation into the color frit of all the elements required in the development of color under our conditions. The frits were fired in the kiln along with the ware, at about Cone 07.

The following formulae show what part of the glaze, whose total RO equals unity, was fritted as the color-introducing compound:

$$6\quad \left.\begin{array}{l}0.08\ K_2O\\0.12\ Na_2O\\0.42\ CaO\end{array}\right\}\ \left.\begin{array}{l}0.03\ Al_2O_3\\0.002\ CrO_3\end{array}\right\}\ \left.\begin{array}{l}0.70\ SiO_2\\0.10\ B_2O_3\end{array}\right\}\ 0.096\ SnO_2$$

$$7\quad \left.\begin{array}{l}0.08\ K_2O\\0.14\ Na_2O\\0.87\ CaO\end{array}\right\}\ \left.\begin{array}{l}0.03\ Al_2O_3\\0.0025\ CrO_3\end{array}\right\}\ \begin{array}{l}0.60\ SiO_2\\0.07\ B_2O_3\end{array}\ +\ 0.107\ SnO_2$$

$$8\quad \left.\begin{array}{l}0.02\ K_2O\\0.05\ Na_2O\\0.25\ CaO\end{array}\right\}\ \left.\begin{array}{l}0.02\ Al_2O_3\\0.002\ CrO_3\end{array}\right\}\ \begin{array}{l}0.53\ SiO_2\\0.06\ B_2O_3\end{array}\ +\ 0.107\ SnO_2$$

Of these three color-frits, number 6, while less stable under reducing influences than those colors based on Seger's

underglaze reds, appeared to give the most desirable effect in terra cotta work of any in my list.

Number seven appears as a slightly more pleasing color effect, though it seemed to a small degree less stable. Number eight gave a quite direct red.

The Stewart kilns having been remodeled during these tests by taking out their closed bottoms, and thus converting them into ordinary down-drafts, it became much more difficult to get purely oxidizing conditions. The colors had now to be fritted in the middle of the setting. In order to make them at all serviceable, the borax had to be increased, thus adding to the stability but detracting from the pleasing tone of the color.

In experimenting to find how much boric acid must be added to meet this change in conditions, I found the two following most stable:

$$9 \quad \left.\begin{matrix}0.075\ Na_2O\\0.840\ CaO\end{matrix}\right\}\ 0.014\ CrO_3\ \left\{\begin{matrix}0.94\ SiO_2\\0.15\ B_2O_3\end{matrix}\right. + 1.00\ SnO_2$$

$$10 \quad \left.\begin{matrix}0.100\ Na_2O\\0.840\ CaO\end{matrix}\right\}\ 0.014\ CrO_3\ \left\{\begin{matrix}0.780\ SiO_2\\0.200\ B_2O_3\end{matrix}\right. + 1.00\ SnO_2$$

Reduction of lime showed weakness of color, from lack of development or reduction.

The introduction of barium to the glaze seemed to give great promise at first, for a content as high as 0.14 equivalents in the glaze appeared to make the color absolutely proof against reducing influences, but a counter trouble arose at once from infusibility of the glaze. When it happened that the ware was rather over-fired at top, a very fine direct red was the result; but in the middle of the kiln the glaze appeared rough and dead, besides being of a bluish purple tinge. Fritting of barium did not seem to help matters any. The addition of 0.06 to 0.08 equivalents did not seem to alter the shade so very much, though it added materially to the stability of the color.

The use of an engobe, under the glaze, was not resorted to very extensively, as I had not at that time worked out a first-class composition. Some trials were made with an engobe specially prepared to aid the glaze to resist reduction, and a great improvement was noted. Barium again was the

most powerful agent, but even without it, the results showed improvement. The best composition was as follows:

$$11 \quad \left.\begin{array}{l} 0.50\ Na_2O \\ 0.10\ K_2O \\ 0.20\ BaO \\ 0.20\ CaO \end{array}\right\} \quad 0.35\ Al_2O_3 \quad \left\{\begin{array}{l} 3.50\ SiO_2 \\ 0.20\ B_2O_3 \end{array}\right.$$

This engobe required a frit, as did all the other serviceable ones.

This closed my experimental work on this interesting subject. I had succeeded in producing a stable red glaze at our heat and under our firing conditions, but it was not of a very attractive or desirable color. It is possible that the low temperatures at which I had to work both for fritting and fusing the glazes made the task peculiarly difficult.

My experiments seemed to me to be reasonably complete on the points involved, and there remained but one line of attack which was still open to us, viz, fritting the color at very high heats. Whether a stable red of good color can be produced at high temperatures, which will still remain good when dissolved in raw glazes fired at low temperature, under such conditions as prevail in roofing tile manufacture, I do not know.

DISCUSSION BY ROSS C. PURDY,

COLUMBUS, OHIO.

Notwithstanding the fact that the manufacture and use of chrome-tin pink has become very general among the potters, and that the formulae and directions for its preparation are given in many books, but little is known of its characteristics and peculiar behavior under varying conditions.

Most potters are very loth to give the slightest information as to how they make and use it, holding their information strictly secret. Very few investigators have published their results on this subject, and when they have, it has nearly always had reference to some minute detail, which alone would not give the reader much of an idea of the subject as a whole. Thus, it is with great interest that

potters and scientific investigators have read what Seger has given us, and later the paper of Hull in Vol. IV of these Transactions. Much valuable information has been published by these two writers, and it is hoped that these publications are but the beginning of a more general discussion on the subject. There surely cannot be any good reason to keep secret data, which, if collected, would undoubetedly result in making clear the exact conditions which govern the most successful manufacture of this useful but comparatively little understood color phenomenon.

The purpose of this note is not to describe any series of experiments made on chrome-tin pink glazes, but to discuss the results of Hull's investigations in the light of my own experience.

Adopting Hull's scheme of approaching the subject, the question of the most favorable composition for the pink frit will receive first attention.

His series were well planned to show the effect of varying amounts of the ingredients on the resultant color stain. Lack of time was undoubtedly the reason why various compounds of lime and chromium were not experimented with, but they should be before such sweeping conclusions as he made, were drawn. For instance, if fluorspar or gypsum were tried as sources of lime or potassium bichromate, lead chromate or chromium sesqui-oxide as sources of the chromium, he would have found a marked contradiction to some of the results as obtained with the ingredients he used.

In the first place he did not approach practical working conditions when he used chromic acid, for potters so far as I know, never use it. Its end reaction on the other ingredients cannot be the same as other chrome compounds are. It is analogous to the proposition of using potassium carbonate in place of feldspar, in any given formula.

It may be argued that the chromic acid would reduce very readily to chromic sesquioxide, and would thus act as when added as such with the advantage of a more thorough distribution throughout the mass of stain. Is the effect of

SiO_2 in a body the same whether added as quartz or soluble silicic acid? [1]

Then again he stepped beyond practical limits when he calcined his "pink frit" at cone 16. Certainly he can obtain pink frits at such a heat, as his own work proves, but that does not justify him in drawing the conclusion that "it is evident that a high temperature is necessary for the development of a frit which will produce a pink color in a glaze."

The writer has made good chrome pink stains or frits both at cone 8 and cone 4, but the *best* were calcined at cone 4. Heats higher than these are obtained in but comparatively few potteries where chrome-tin pink is produced to any extent, yet they are meeting with good success.

From what he writes, it would be inferred that he calcined his pink stain but once. It is the general experience that recalcination, after having washed and re-ground, produces the best results; hence, it is the general practice now to re-calcine the stain.

In choosing his best composition for a chrome-tin pink stain be takes into account the following consideration: [2]

"The tin might be raised without detriment to the frit itself, but it is not considered desirable to introduce more tin than is necessary into soft fire glazes. It may be that the tin could be decreased, but the results of these experiments do not indicate that this could be done to any considerable extent."

To compare my results with Hull's, five of the best stains (frits) which were calcined at cone 4 in 48 hours are tabulated on page 252.

The per cent. of SnO_2 in Hull's No. 15 stain, Series A-III is 52.3+ and of No. 16, 47.3 per cent. The average of these two stains agrees in the percentage amount of SnO_2 with those found in the table on page 252.

His statements as to the limits of variation in the amount of SnO_2 best to use in a stain are very true. Neither his nor my results would indicate that the tin could be decreased to any considerable extent. And if 50 per cent. SnO_2 will

1 Transactions Am. Ceramic Society, Vol. III, page 25.
2 Ibid, Vol. IV, page 286.

CHROME-TIN PINK FORMULAE.

No.	Per Cent.	Chem. Formula with RO as Unity.					Chem. Formula with SnO_2 as Unity.					Results in a Standard Glaze.
	SnO_2	RO	SnO_2	Cr_2O_3	SiO_2	B_2O_3	RO	SnO_2	Cr_2O_3	SiO_2	B_2O_3	
1	55.56	1.0	1.240	0 038	1.110	0.078	.0315 Na_2O .0806 K_2O .7444 CaO .8065	1.0	0.0306	0.893	0.03	Brownish, hard to disseminate in glaze
2	50.31	1.0	1.111	0.009	1.104	.....	.8825 CaO .0173 PbO .8998	1.0	0.0086	0.992	.....	Good Maroon.
3	50 2	1.0	1.127	0.013	1.104	0.009	.0084 Kn_2O .8638 CaO .0149 PbO .8871	1.0	0.0115	0.9795	0.0084	Dark Maroon.
4	50.5	1.0	0.744	0.016	0.325	.....	1.1252 CaO .2187 Na_2O 1.3439	1.0	0.0215	0.437	.....	Blood Red.
5	50.5	1.0	1.000	0.066	0.978	0.008	.0040 K_2O .0041 Na_2O .8634 CaO .1284 PbO .9999	1.0	0.066	0.9787	0.0083	Pink.

suffice, of course it is not desirable to introduce more. But it is not desirable principally for the economic reason, and not because it is objectionable in a soft-fire glaze.

In his fourth consideration, he justifies Seger's use of more tin, because his stain was intended for higher fire glaze; and assumes that amount of tin would be detrimental to a soft-fire glaze.

What is the office of tin in a glaze, and does it enter into the glaze composition chemically, so as to materially alter its pyro-chemical behavior?

S. Burt[1] proved in very pretty and unique experiments that tin is slightly soluble in silicates and seemed justified in concluding that the soft-fire glazes do take some tin into solution. But he proved also that but very little was dissolved or at least but little remained in solution. The bulk of the tin, even when only 0.02 equivalent was added to the glaze, remained undissolved.

Stover[2] gives a formula of a cone 12 glaze where .04 equivalent SnO_2 has not gone into solution.

We are safe then in concluding that not much tin goes into solution whether added raw, and in a frit, or in a stain, at a high or low heat.

The question then is how much tin can we add without detriment to a soft fire glaze? I do not know. I have used 0.2 equivalent SnO_2 for ordinary fritted white enamel of practically the same composition as the red glaze, and made to fire on the same ware. I have never used a chrome-tin glaze having over 0.18 equivalent SnO_2, and have used successfully as low as 0.075 equivalent SnO_2, the best success being one with only 0.10 equivalent SnO_2.

The question of the use of more or less tin in a stain is not, then, one of its adaptability to a soft or hard fire glaze, but rather one of necessity and economy.

For example, take stain four in the above table; 0.1 equivalent (SnO_2-unity) gave a very deep red with good body to the color. With double that quantity, I did not get a desirable color effect, but the glaze was not impaired. Now,

1 Trans. American Ceramic Society, Vol. IV, page 139.
2 Trans. American Ceramic Society, Vol. IV, page 144.

if there is sufficient tin in that stain to produce the desired color and covering power by the use of only 0.1 equivalent, it does no seem necessary to use more.

From Burt's experiments before quoted, we learn then, that the office of SnO_2 is not that of a flux or acid, for it does not go into combination with the glaze, but that it remains inert and suspended. Therefore, if the stain is a stannate, as some suppose, it would not go into solution, and, as the above illustration shows, the pyro-chemical behavior of the glazes would not be materially affected as Hull supposed, when he says on page 243 "Naturally more flux was required to produce a mature glaze than would be necessary if less of the pink frits were used."

His second consideration was "Series A-I indicates that the chromic oxids (0.0375) is as high as is allowable."

Stain No. 5 above given has 0.066, and yet it was not green and the color effect in the glaze was a good red. Therefore, the use of more than 0.0375 Cr_2O_3 is *allowable*, but *not always justifiable*.

His third consideration "The lime was purposely made as high as Series A-III would warrant," leaves no basis for criticism, for he affirms nothing except what his trials would warrant. But, it will be noticed that my stain No. 4 contains 1.125 CaO which is 0.125 more than Hull's trials would warrant.

His further trials, Vol. IV, page 252, also show as he says in his conclusion "that a wide range in the quantity of lime used in the frit is allowable."

These differences in results between Hull's and my investigations are due principally to the fact that I used various compounds of lime and chromium and burned the frit or stain at a lower heat.

There is only one criticism to be made on the glaze series and that involves a very essential point. In every case, except in Series B, where the effect from increasing the content of boracic acid in a glaze was sought, the boracic acid was added *at the expense of* the silica. It is the experience of every glaze maker with whom I have discussed this point, that the silica should be as high as possible, for

it has a beneficial effect on the chrome-tin color. Therefore, while he was increasing a factor which he knew was detrimental, he was doing so at the expense of another factor which was beneficial. This accounts in a great measure for the fact that his most favorable combination of lead and boracic acid as described on page 251, Vol. IV, does not accord with the writer's experience.

The ratio of lime to the alkalies is also of decided importance. The highest proportion he used was 2 : 1, while it is the writer's experience that about 3 : 1 is the best ratio. By the use of a larger ratio, some of the lime can be added as carbonate, which the writer has found desirable for the better maturity of the glaze in general, and, in this case, owing to the increased lime, to this color in particular.

After considerable experimenting, the writer found that the glaze best suited for the production of chrome-tin pinks and reds was:

$$\left.\begin{array}{l}0.22\ PbO\\0.59\ CaO\\0.19\ KNaO\end{array}\right\}\left.\begin{array}{l}0.16\ Al_2O_3\\0.X\ SnO_2\\0.Y\ Cr_2O_3\end{array}\right\}\begin{array}{l}2.10\ SiO_2\\0.475\ B_2O_3\end{array}$$

If the ordinates and abscissae of the lead and boracic acid were plotted on Hull's diagram given on page 251, Vol. IV, it would be found that the above glaze is more than 0.1 equivalent B_2O_3 within the area in which he says good results are improbable, and more than 0.175 equivalent beyond the limits within which he obtained his best results.

A METHOD FOR MAKING ENAMELED BRICK.

BY

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In 1900 the value of the enameled brick output of the United States was $323,630.00 while in 1901 it had increased to $463,709.00, a gain of about 43 per cent. That this increase was not even greater can be attributed to the comparatively high price of this product, for enameled brick have of late come into greater command in building construction. For use in wainscotings and wall surfaces in depots, hotels, or in any public building, or passage way; in dairies, bathhouses, power stations, etc., where cleanliness is desired; in elevator shafts, subways, etc., where reflected light is needed; or in any construction requiring an ornamental, smooth, bright, sanitary surface, the enameled clay surface has no satisfactory substitute. And of course for such uses the white enameled brick is almost invariably used and white is usually understood when reference is made to simply "enameled brick." There is a limited demand for colors for interior use, as borders or in oriental work or hearths, but the bulk of a manufacturers output is ordinarily white.

A great many enameled brick have in the past been imported from England, our own supply of first-class product being inadequate to the demand. In the past few years, however, a number of American manufacturers have offered the market very creditable ware. Yet, while the ware is first-class, the high price of enameled brick undoubtedly prevents their specification in many instances where their use is strongly desired.

Two years ago it was the writer's good fortune to be assigned the task of producing a white enameled brick which would be of good quality but moderate in price. The experiments made to accomplish this end brought out many

interesting points while leading to very satisfactory results. Believing that the mode of procedure in the work as well as the results attained and incidental facts brought out may be of interest and possibly of value to the members of this society it will be the purpose of this paper to trace the work from beginning to end.

THE BODY:

The enamel was to be applied to the following fire-clay. It is known as the Lower Mercer clay, occurring in a vein from six to twelve feet thick, and won by mining. An analysis of this clay gave:

SiO_2	Free	24.20
	Combined	34.66
Al_2O_3		27.96
Fe_2O_3		1.40
CaO		0.51
MgO		0.64
Alkalies		2.78
Combined water		8.82
		100.92

This clay burned to a hard, dense, body, light buff in color, at cone 8. The question as to whether to apply the enamel to the green or to burned ware naturally was amongst the first to arise. To coat dry-press brick in the green state with a glaze or enamel is a delicate process entailing much loss and a poor quality of product. As the product of the plant was almost exclusively dry-press brick the making of mud brick for the reception of an enamel would have required much new machinery. These considerations swung the choice towards a two-fire product and the fact that in the course of burning there were always more or less soft-burned brick which were unsalable except as culls, led to the decision to use these soft-burned brick as biscuit for the enamel. To use at the start bricks which would not sell as builders, merely because not hard enough, seemed the first step towards securing a moderate-priced enameled brick. Of course, the supply of these soft brick was limited but would prove ample for a start and for small

sales. It would not be necessary to make a special biscuit burn except in case of a much larger run on enameled brick than was at that time in sight. Furthermore, upon consideration of the care, time and loss in enameling green ware and burning in one fire, it was estimated that the difference in the cost of burning would be off-set by the ease and rapidity of handling, and small loss attendant upon a two-fire product.

The soft burned bricks to be used as biscuit had shrunk on the average, in volume, 8.2 per cent of the die measure. The hard burned bricks of the same clay usually shrunk 13.4 per cent, so that the body used had received about 61.2 per cent, of its total shrinkage. The bricks were soft and punky and of course very porous.

THE GLAZE.

White enameled brick are made either by applying a true enamel (usually tin) to the body or by using a clear, transparent glaze over a white engobe which completely conceals the true body color. As a tin enamel requires a calcareous clay for the best results, and as the clay to be used was a fire-clay with but little lime, it was decided the engobe-glaze method would offer fewer difficulties and better ware.

To insure durability, enameled brick must have a hard-burned body and an enamel which has been burned fast to the body, is well matured and is free from crazing or shivering. When used in light shafts or in other outside work, severe weather changes soon destroy a poorly enameled surface. The chance of water entering the wall and consequent freezing and thawing breaking off the enamel must be made practically impossible.

In view of these requirements, a glaze of the porcelain type was selected as desirable, to mature at the point where the body became very hard and dense, *i. e.* cone 8. The porcelain glazes are very durable and not as prone to craze as lower fire glazes.

Using the formula of Seger cone 4 as a starting point a series of glazes was made the extremes of which it was

certain would be too soft and too hard respectively for this heat.

This series was as follows:

SERIES I.

No.	K_2O	CaO	PbO	Al_2O_3	SiO_2
1	0.3	0.4	0.3	0.5	4.00
2	0.3	0.5	0.2	0.5	4.00
3	0.3	0.6	0.1	0.5	4.00
4	0.3	0.7	0.0	0.5	4.00
5	0.3	0.7	0.0	0.6	4.80
6	0.3	0.7	0.0	0.7	5.60

The use of lead above the temperature which will turn cone number 1 is not recommended by Seger. The writer, however, has used lead with fair success at cone number 3 and had heard of its use in porcelain glazes, the idea being that even if the lead did volatilize, it would start good fusion before going. Since making these experiments the writer has actually observed in a large plant the use of lead in a porcelain glaze fired at cone 8–9†. With not much faith however, that lead could be used at this temperature to advantage, it was decided to try it in order to locate the type of glaze needed by showing a lead glaze not feasible. The glazes numbers 5 and 6 it was almost certain would be too hard.

The glazes were made up in about three pound batches, ground in a small jar mill with flint pebbles, put through a 120 mesh sieve and excess water siphoned off until a Baume' hydrometer dropped into the slip gave a reading of 40, this thickness having been found to work well on the soft body. Soft burned buff brick were dipped with the glaze and burned in saggers at cone number 8 in large down-draft kilns, amongst other ware.

†This glaze, while bright, smooth and clear is subject to crazing.

The results of the test of this series were as follows:

No.	Condition of Fired Glaze.
1	Thin—Brick dry in places—Crazed.
2	Thin—Brick dry in places—Crazed.
3	Thin—Crazed.
4	Crazed slightly—Not entirely clear—Smooth and fairly bright.
5	Milky—Not melted to clearness.
6	Milky—Not as clear as No. 5.

From the foregoing it is evident that a glaze of the type of number 4 was needed but one that should be somewhat softer.

Seger* has shown that cone number 4 is the most fusible glaze of the porcelain type. To secure greater fusibility therefore, other ingredients must be introduced than those indicated in the cone number 4 formula.

A complexity of bases, using magnesia and zinc, was next tried, keeping the alumina and silica constant. The series was as follows:

SERIES II.

No.	K_2O	CaO	MgO	ZnO	Al_2O_3	SiO_2
7	0.3	0.6	0.1	....	0.5	4.00
8	0.3	0.5	0.2	....	0.5	4.00
9	0.3	0.4	0.3	....	0.5	4.00
10	0.3	0.3	0.4	....	0.5	4.00
11	0.3	0.4	0.25	0.05	0.5	4.00
12	0.3	0.4	0.2	0.10	0.5	4.00
13	0.3	0.6	0.0	0.10	0.5	4.00

The glazes were treated the same as those in Series I and applied to buff bricks and burned in large kilns to cone number 8 with the following results:

No.	Condition of Fired Glaze	
7	Milky.	
8	Milky—Not as fusible as No. 7.	All far from being well matured.
9	Milky—Not as fusible as No. 8.	
10	Milky—Not as fusible as No. 9.	
11	Cloudy—But better than any of the above.	
12	Cloudy—But better than No. 11—Smooth, glossy.	
13	Almost clear—Smooth and bright.	

*Translations of Seger's Collected Writings Vol. I, Pg. 241.

From this series it is clearly seen that magnesia is a harder flux than lime and if used to replace the latter the glaze is hardened thereby. This bears out Seger's statement as to the order of fluxes. The replacement of a little magnesia by zinc gave a correspondingly small improvement. The dropping of magnesia entirely and the substitution of 0.1 equivalent of zinc oxide for 0.1 equivalent of calcium oxide in glaze No. 13 immediately gave a glaze far superior to any of the lime-magnesia glazes, and also better than the lime-potash glaze, No. 4, indicating that zinc has greater fluxing power than lime or magnesia, or than a combination of these.

The failure of a complexity of bases in this series to give more fusible glazes does not harmonize with Cannan's* results with raw lead glazes, nor with his conclusions with regard to the fluxing power of magnesia. Where Cannan, working at a temperature of cone 04-02, secured better glazes the greater the number of oxides he used to make up his RO, and with magnesia obtained a higher single replacement of PbO than with any other oxide, the writer, at cone 6-8 finds a complexity of bases, using magnesia, very unsatisfactory, and that any replacement of CaO by MgO in a porcelain type glaze at once hardens the glaze.

Glaze No. 13 being the clearest and brightest glaze so far, developed, but this still being a trifle hard, it was decided to next run a series of lime-zinc glazes and also to try replacing zinc with barium.

This was done as follows:

SERIES III.

No.	K_2O	CaO	ZnO	BaO	Al_2O_3	SiO_2
14	0.3	0.65	0.05	....	0.5	4.00
15	0.3	0.65	0.05	....	0.5	3.80
16	0.3	0.65	0.05	....	0.5	3.50
17	0.3	0.60	0.10	....	0.4	4.00
18	9.3	0.60	0.10	....	0.4	3.80
19	0.3	0.65	....	0.05	0.5	4.00
20	0.3	0.60	....	0.10	0.5	4.00
21	0.3	0.60	....	0.10	0.4	4.00

*Trans. Am. Cer. Soc. Vol. III, 126.

The results of this test are shown below:

14, 15, 16, 17, 18	All were smooth, bright glazes but No. 18 showed best maturity, the others holding more or less bubbles in suspension. No. 18 was slightly crazed in corners.
19, 20, 21	Not noticeably different in appearance from the corresponding zinc glazes but all were badly crazed.

Number 18 being almost entirely satisfactory, the flint was increased to rid the glaze of crazing. It was found that this was accomplished by a very small addition, and the glaze finally accepted as being suitable for the work had the formula:

$$22\left\{\begin{array}{l} 0.3\ K_2O \\ 0.6\ CaO \\ 0.1\ ZnO \\ \hline 1.0\ RO \end{array}\right\}\ 0.4\ Al_2O_3\ \left\{\ 3.825\ SiO_2\right.$$

The foregoing experiments with magnesia resulting so differently from Cannan's, another trial was made to verify the previous work. This time magnesia was used to replace lime in glaze No. 22, a glaze known to work well at this temperature. The introduction of burned, slaked and bolted dolomitic limestone was also attempted as a source of lime and magnesia.

To this end the glazes were made up replacing first 0.2 and then 0.3 equivalents of the lime in glaze No. 22 with magnesia, as follows:

No.	K_2O	CaO	MgO	ZnO	Al_2O_3	SiO_2
23	0.3	0.4	0.2	0.1	0.4	3.825
24	0.3	0.3	0.3	0.1	0.4	3.825

Furthermore, dolomitic limestone, prepared as noted above and secured from the same dealers as furnished Cannan's supply, was used in two glazes. In one glaze, the 0.6 equivalents of CaO called for in glaze No. 22 was replaced by 0.6 equivalents of limestone, while in the other the amount of whiting called for in the batch receipt of No. 22 glaze was

replaced pound for pound by dolomitic limestone. The limestone in question, after burning, gave on analysis*:—

$$\begin{array}{l} .51\ Ca\ (OH)_2 \\ .49\ Mg\ (OH)_2 \\ \hline 1.00 \end{array}$$

Thus the limestone glazes were composed as follows:

No.	K_2O	Limestone.		ZnO	Al_2O_3	SiO_2
		CaO	MgO			
25	0.300	0.306	0.294	0.100	0.400	3.825
26	0.228	0.354	0.341	0.076	0.305	2.962

Glazes Nos. 23, 24, 25 and 26 were now applied to light buff bricks in the usual way and trials placed at the top, middle and bottom of a small down-draft kiln set with enameled brick. The kiln was burned off at the usual temperature, cone 8 in the top and cone 6 in the bottom. On cooling and drawing the kiln, the results of the test were found to be as shown below:

Glaze No.	Top of Kiln.	Middle of Kiln.	Bottom of Kiln.
23	Melted to smoothness but dull, milky and full of bubbles. Far from good fusion.	About same as top.	Very imperfectly fused. Dull and milky.
24	Very cloudy and imperfectly fused. Similar to No. 23.	About same as top.	Barely smooth. Some of the trials not fused sufficiently to give smooth surface.
25	Fairly smooth and glossy but very poor fusion—glaze cloudy thoughout.	About same as top.	Poor fusion—rough and no gloss—not much more than a sintered mass.
26	Not as good as No. 25.	Mostly rough and not fused at all except a few near bag.	Not as good as No. 25.

In addition to securing the desired glaze for the work in

*Trans. Am. Cer. Soc. Vol. III, 127.

hand, the above experiments brought out the following facts:—

(a.) Neither magnesia nor dolomitic limestone offer any encouragement for their use as fluxes in a glaze of this type and maturing temperature.

(b.) Magnesia is below lime in value as a fluxing agent at this temperature.

(c.) Zinc has greater fluxing power than lime.

(d.) That barium has at least equal fluxing power with zinc but gives rise to crazing if substituted for zinc in a zinc-lime glaze free from crazing.

THE ENGOBE.

Having perfected a satisfactory clear glaze, as given in No. 22, the next step was to secure a white engobe to apply between the body and glaze The engobe must be white, burn reasonably hard at the temperature used and shrink just enough to neither shell off, nor crack nor curl up.

Langenbeck* cites the case of a pure china clay being used as an engobe for enameled brick burned in single fire at cone 9-10. At the temperature at which it was desired to fire, however, a china clay in this case would be too soft and shrink to much for the body shrinkage. To reduce the shrinkage, flint is the best agent while for hardening the engobe feldspar is most suitable. With these three materials therefore, a series of engobes was made up varying the proportions as follows:

No.	China Clay.	Flint.	Feldspar.	Formula.		
				K_2O	Al_2O_3	SiO_2
1	75.00	0.00	25.00	.135	1.00	2.55
2	75.00	15.00	10.00	.058	1.00	3.03
3	60.00	25.00	15.00	.104	1.00	4.02
4	50.00	30.00	20.00	.156	1.00	4.80

The slips were blunged by hand, put through a 40 mesh sieve, and then through a 100 mesh sieve. By experiment it was found that the best working thickness was

*Chemistry of Pottery—Pg 144.

Baume 45°. On coating a number of bricks with the above engobes and burning, the following results were noted:

No. 1 had cracked slightly.

No. 2 was soft.

Nos. 3 and 4 were both good, but No. 4 was harder and hence preferable.

No. 4 was the engobe chosen for use.

In a paper entitled "Stoneware Slips or Engobes" Geijsbeek† speaks of obtaining results by substituting one clay for another in an engobe mixture. This seems a long way to take to get the proper engobe. After one trial of 25 per cent flint and 75 per cent china clay, Geijsbeek gives up flint and says "as it seemed strange that flint could not be used so far in an engobe. I have started experiments on that subject, but as yet I have been unable to finish same or to come to any conclusion." That he will find flint a valuable agent in engobe compounding, I do not doubt at all. Using one clay, and with flint and feldspar as controlling agents for shrinkage and hardness, a wide range of engobes can be made, and this seems a more exact method than to shift clays.

In order to increase the tenacity with which the engobe holds to the body, five per cent. of sodium carbonate was used in the engobe, being added as a solution, in the least possible amount of water, after the engobe had been made up to the proper thickness. This expedient is the practice among stoneware men to prevent crawling of engobes and glazes, borax and calcium sulphate being also used for the same purpose.

On making a trial of No. 4 engobe with No. 22 glaze, a very white, smooth and bright enamel was obtained.

While experimenting with engobes, a few colors were attempted as follows:

A blue frit was made up corresponding to the formula

$$\left.\begin{array}{l}0.50\ Na_2O\\0.50\ CoO\end{array}\right\}2.00\ SiO_2$$

and melted in flint-lined crucibles in a blacksmith's forge. The resulting glass, which was a very intense blue, was

†Trans. Am. Cer. Soc. Vol. IV—48

ground to 120 mesh fineness in jar mills, the water dried off, and the powdered frit boxed for use. On adding a little of this blue frit to engobe No. 4, a very good blue engobe was obtained, which, covered with No. 22 glaze, gave a very acceptable blue enamel. Two shades were made as follows:

Blue No. 1 (Light)	97.5 parts engobe No. 4. 2.5 parts blue frit.
Blue No. 2 (Medium)	95.2 parts engobe No. 4. 4.8 parts blue frit.

Another method of securing a blue engobe was to add a cobalt solution direct to the engobe No. 4, the latter being sufficiently alkaline to precipitate the cobalt throughout the mass, and give a very uniform coloring in the resulting engobe. By adding 770 c. c. of a ten per cent. $CoCl_2$ solution to each one hundred pounds (dry weight) of engobe, a very delicate, pretty blue is obtained. This is equivalent to 0.10 per cent. CoO. When the cobalt solution is added the cobalt is precipitated as a carbonate, thus leaving sodium chloride where before we had sodium carbonate in solution in the engobe. This does not affect matters seriously, however, as the chloride is almost as efficient as the carbonate as a fastening or binding agent. At any rate, the colored engobes adhere to the body as tenaciously as the white engobe. Neither white or colored engobes at any time gave trouble from crawling.

This process of precipitation does away with fritting and as cobalt chloride is no more expensive than a good grade of the oxide, the process is also much cheaper. Iron colors also can be obtained by the precipitation method.

A very good chrome-green was obtained by adding raw chromic oxide to No. 4 engobe, in the proportion of thirteen per cent. oxide to eighty-seven per cent. engobe. This gave a rather dark sage-green enamel. Other shades were not attempted, as colored enamels were not considered of sufficient importance.

A very satisfactory pink engobe was made by using a pink stain or frit, made in accordance with Seger's formula, to the amount of eight or ten per cent. in the white engobe.

In attempting to use copper for sea-green effects, the color was obtained successfully except around the edges of

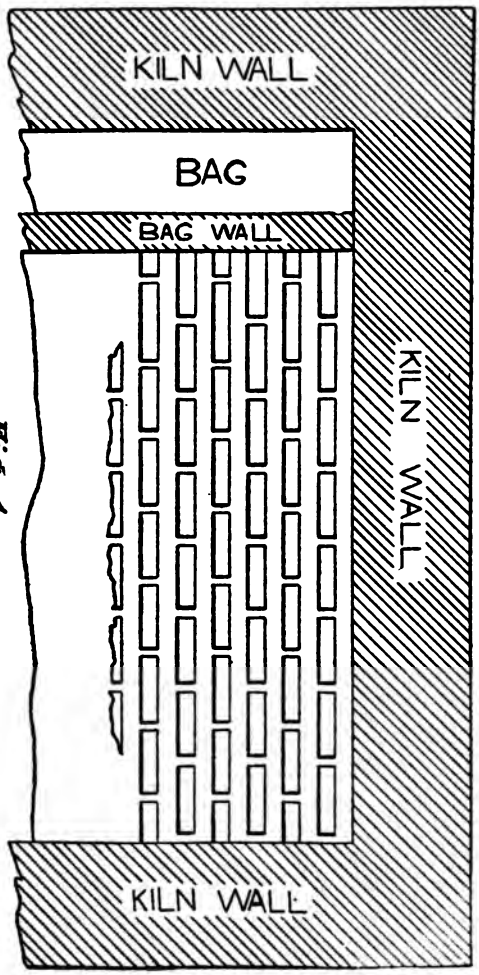

Fig. 1
Showing checker-work of brick set on edge on kiln floor for reception of bench of enameled brick.

the brick, where the copper volatized leaving a white edging entirely around the enameled face. This defect was sufficient to bar out copper-greens. On breaking a brick coated with copper-green enamel, it was noticed that the color had left the engobe entirely, and what really existed was a *green glass over a white engobe.* The copper had evidently tried hard to get away, but the heat and duration of burn were not sufficient to expel it entirely.

A fairly good brown engobe was obtained by the use of 12 parts of commercial manganese to 88 parts of white engobe.

This manganese gave an analysis:

MnO_2	81.01
Fe_2O_3	5.73
Silicious residue	13.26
	100.00

Uranium colors were a failure, coming out a dirty green in every one of several tests.

All colors were obtained by making the colored engobe and then treating and applying to the ware just as was done with the white engobe. Special attention had to be paid to location of colors in the kiln however, as will be noted later.

In general, it may be said, that having secured a satisfactory white engobe, colored engobes may be easily obtained by the introduction of coloring oxides or colored frits. The small proportion of the coloring agent usually required produces no physical defects in the engobe. A good white engobe will stand in some cases fifteen per cent. of a coloring oxide, and still burn as smooth and perfect as originally.

USE OF A GREY BODY.

An attempt was made to use the white enamel as above developed on light grey brick, in order to increase the available biscuit supply, but on trial it was found that the manganese in the grey body was sufficient to color the engobe and darken the glaze to such an extent as to make the product unsalable as a good white enameled brick.

DIPPING AND CLEANING.

The glaze and engobe had now been developed in an experimental way, and the process of making the brick on a

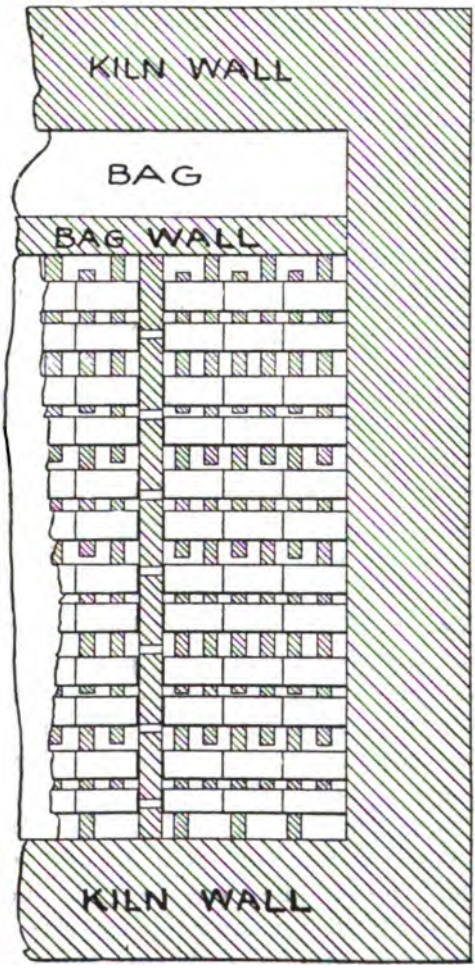

Fig. 2.
Showing how enameled brick are built up on checker-work.
Starting of a three brick bench.

manufacturing scale was taken up as the next step. The experiments for a white enamel had scarcely been completed, when an order was received for 35,000 white enameled brick for a light-shaft in an office building. The brick made by the following method, therefore, were for an actual order and every effort was made to devise a rapid and economical method.

Soft-burned light buff brick were set apart by the kiln drawers and sent to the enameling room, a small brick building which had formerly been used as an oil storage house, but was left vacant when the use of oil as a fuel was discontinued. Here the brick were laid face up on a long bench, and subjected first to the cleaning process, consisting of going over the face to be enameled with an iron scraper to remove adhering sand, and then with a stiff brush to take off all further dirt or dust. This required but little time. Two men could easily go over 100 brick a minute. The bricks were then dipped into the engobe two at a time, the dipper holding one in each hand. One man dipped into the engobe and about twenty brick behind him came the glaze dipper who had helped with the engobe until the two operations were sufficiently spaced. By experiment it was found that by working with the engobe at a thickness of 45° and the glaze at a thickness of 50° (Baume) and dipping into the glaze a few moments after the application of the engobe, a very smooth uniform coat resulted, free from pin-holes or bubbles. Both glaze and engobe were prepared as in the preliminary experimental work. The glaze and engobe were both occasionally stirred up by means of wooden paddles kept near the dipping tubs. Following the glaze dipper as close as he could conveniently work, came the cleaner who removed the excess engobe and glaze with a large spatula. With one stroke the excess coatings along one side could usually be nicely sliced off so that only a few strokes to each brick were required. By this means it was found the brick could be more easily, rapidly and effectually cleaned than by allowing the coatings to dry and then brushing off superfluous material as is the practice in many plants.

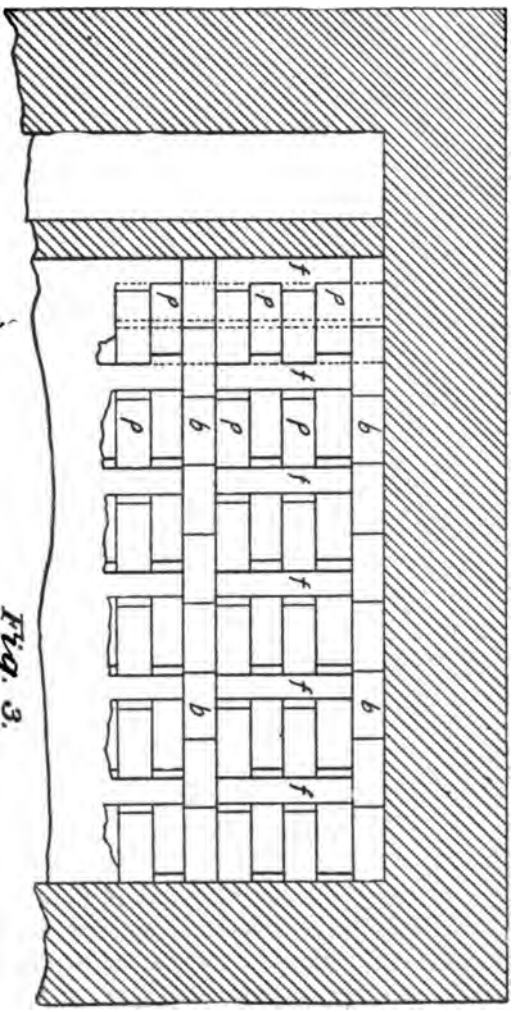

Fig. 3.

Showing binding course for single bench. Dotted lines indicate enameled brick below.

b - Binding course running clear across kiln.
p - Brick protecting enamel spaces. . . .
f - Flue spaces; . . . . . .

In dipping headers, quoins or shapes, such as bull-noses, octagons, etc.. the dipping process was necessarily slower, the brick being dipped one at a time and requiring more care to get the surface properly covered.

The dipped and cleaned brick were now removed from the bench to pallets holding eight brick each and removed to the storage shed just back of the dipping room.

SETTING AND BURNING.

Burning was accomplished in small rectangular down-draft kilns (designated on account of their size "baby kilns") having twin fire-boxes. At first saggers were used, the brick being stood on end, twelve in a sagger. In this way only 1500 brick could be set in each kiln; many saggers were broken from the great weight on them, and setting and drawing a kiln involved much labor. It was seen at once that this system was too expensive. A scheme was then devised of setting the brick without saggers, by building up the bricks so that the fire gases did not come in contact with the enameled surfaces. The method adopted was this: The brick were brought from the storage room on pallets, and set down on the kiln floor. The distance from the storage shed to kiln was only from ten to fifteen feet and one carrier could easily keep two setters supplied. The setters first set burned, unglazed bricks on edge on the kiln floor in rows running parallel to the kiln door and extending clear across the kiln, the rows being about 1½ or 2 feet apart. (See Fig. 1, p. 267). This was for the purpose primarily of allowing the kiln gases free play at the kiln bottom, and ready access to the flues, and secondarily to keep the enameled bricks from the floor, where they would come into contact with sand and ash. After a distance of three brick lengths from the rear kiln wall was reached, the enameled brick were then set on flat on this checker course and built up in walls running parallel to the fire bags. The first course above the checker work however, was usually set in burned, unglazed brick. The walls of enameled brick were so spaced that there was a narrow space and then a wider space alternating. The enameled faces of the brick formed

TRANSACTIONS AM. CERAMIC SOC. VOL. V BARRINGER. ENAMEL BRICK

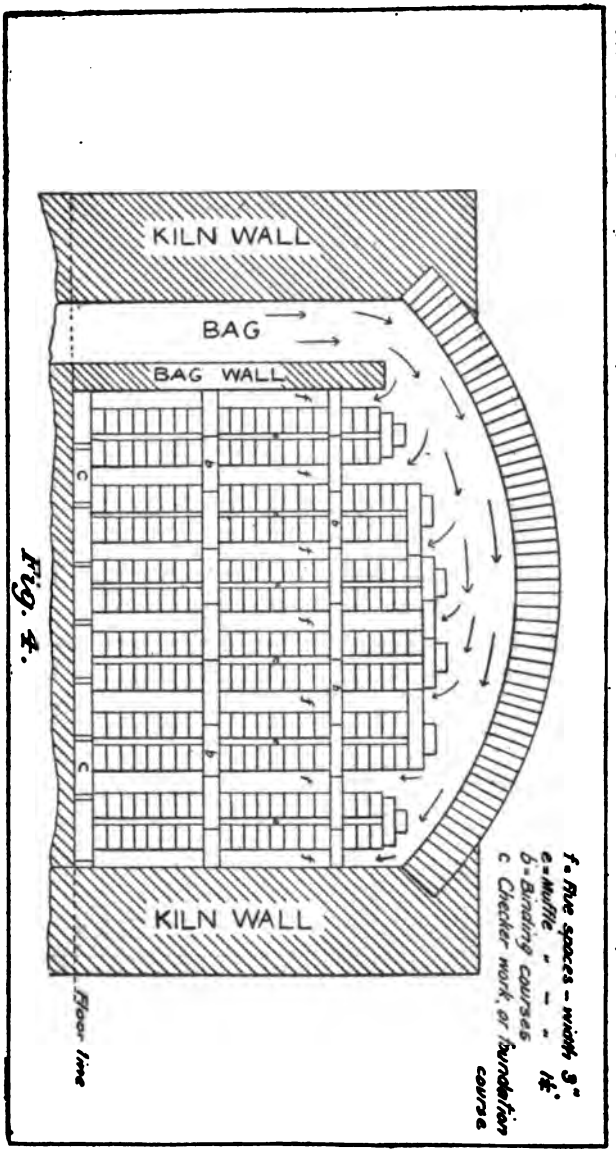

Fig. 4.

the narrower spaces, and the backs of the brick formed the wider spaces, the latter constituting the flues through which the fire gases passed. (See Fig. 2, p. 269). When a height of eight bricks had been reached in the walls, a "binding" course of unenameled bricks was then set to hold the columns straight and prevent rolling. The narrow spaces on which the enameled faces fronted were covered completely, while the larger flue spaces were left open except at intervals where the true binding course ran clear across the kiln. In this way a muffle was built up to protect the enameled faces. (See Fig. 3, p. 271). As the top of the kiln was approached, the middle walls were built higher than the side walls to follow roughly the curve of the crown. When the top had been reached, a final binding and protecting course was put on as shown in figure 4 (see page 273).

The brick were built up in three brick benches.

In setting quoins, bull-noses or shapes, triple walls were used instead of the double wall arrangement, the middle wall being used for these shapes and being protected completely from the fire gases by the two side walls of ordinary enameled stretchers. (See Fig. 5).

To space the bricks, a wooden form termed by the setters a "goose-yoke" was used. This was simply a U-shaped wooden frame and was used in starting from each binding course.

TRANSACTIONS AM. CERAMIC SOC VOL V BARRINGER ENAMEL BRICK

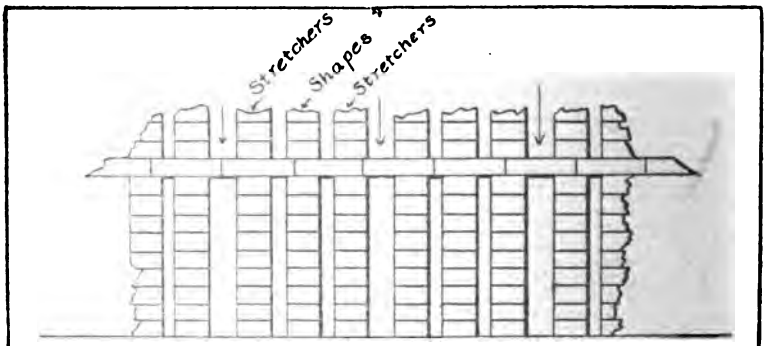

Fig. 5.

Showing method of protecting enameled quoins, bull-noses, etc. by setting between two walls of ordinary enameled stretchers

FIG 6. THE DIPPING HOUSE AND KILNS.

FIG. 7. THE "BABY" KILNS.

By setting in this manner, the capacity of the kiln was increased to 3000, and the labor of setting and drawing was much reduced.

In setting, pyrometric cones were placed in convenient places at top and bottom of the kiln, numbers 6, 7 and 8 being used in the bottom, and 7, 8 and 9 in the top. Peep holes in each end of the kiln were made, so that the cones could be easily seen.

Three men could start in the morning, set a kiln of enameled brick in this fashion, build the wicket and have the fire started in the evening.

Burning usually required from 66 to 72 hours, divided into three periods:

(a.) Slow heating up by wood fires on the floors of all fire boxes, 12 hours.

(b.) Coal fires on bars in all furnaces; firing every hour for 12 hours in all fire boxes, then in alternate boxes every half hour for 12 hours, and finally every twenty minutes in alternate furnaces, carrying the heat up as fast as possible until cone 8 was turned slightly in the top, which usually required 24 hours of the twenty-minute firing.

(c.) Soaking or distribution stage. After cone 8 had been turned on top, which usually occurred before cone 6 in the bottom was affected, the heat was held as steady as possible by careful firing until a nearly uniform temperature was attained throughout the kiln. In the course of from six to twelve hours, cone 6 and often cone 7 would go over in the bottom while there had been no material change in the top. This was deemed the finishing point, and firing ceased. Cone 8 could not be turned in the bottom without overfiring the top.

The finishing point was determined solely with pyrometric cones. At first it was the practice to draw trials towards the close of the burn, but later the cones alone were relied upon, and never failed the purpose. If number 9 cone was melted down in the top, we knew what to expect from the top on drawing the kiln—blistered body and thinned glaze. If the proper cones were not melted in the bottom, dull-finish enamel could confidently be expected. On be-

coming acquainted with the merits of the cone by actual practice, the burners took to watching them carefully, and handled the kilns to get the cones down as uniformly as possible.

Reduction was not resorted to in burning, and oxidizing conditions prevailed throughout most of the burn.

Immediately after firing ceased, the dampers were shoved in and the holes in the kiln crown were uncovered. This took much of the heat from the kiln, without sending cold air in amongst the ware. After about twenty-four hours, dampers were pulled and fire doors opened. The wicket was pulled down after another twenty-four hours, and the kiln cooled as rapidly as possible, the drawing crew usually being able to take out the ware twenty-four hours after the wicket was down. At first, more rapid cooling was tried by leaving dampers out, tops off, and fire doors open immediately after firing ceased, but this resulted in "cooling checks", consisting of long straight cracks extending clear across the enameled face. The practice was gradually changed, therefore, to that above, which gave practically no cracked enamel.

In burning colors it was found that blue and manganese iron brown were comparatively easy to burn. The coloring agents acted as fluxes, and fused partially into the glaze, giving a very uniform shade. In chrome green and chrome-tin pink, however, the glaze must be perfectly matured in itself to give a clear, bright color, as it receives no aid from the engobes colored with these agents. On breaking a brick enameled in green or pink, the line of contact between engobe and glaze was very sharply defined and the color had not fluxed into the glaze covering. It was always necessary for the best results therefore, to burn pink and chrome green in the hottest portion of the kiln, while the other colors could be placed in almost any section.

DRAWING, SORTING AND SHIPPING.

The kilns were drawn by running an ordinary brick car to the kiln, and loading on the enameled brick just as is done with face brick. Not much more care was used with the en-

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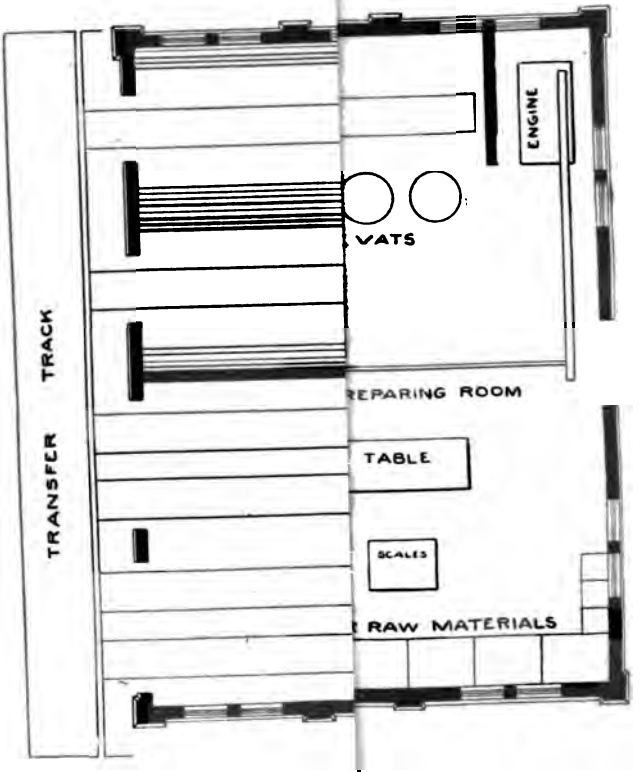
TRANS. AMER. CERAMIC SOC. BARRINGER



ENAMELED BRICK PLANT

APACITY 5000 STANDARD BRICK

SCALE 1"

ameled brick than with unenameled brick. The cars were run to the stock sheds, and the brick sorted under ground glass sky-lights. The color was usually very uniform, and sorting resolved itself into merely removing defective brick, such as contained specks, cracks, broken corners or unusually rough edges. Only two sorts were recognized—"firsts" and "seconds". The "firsts" averaged 80 per cent.

In shipping, the bricks were packed in straw in cars, just as is the custom with face brick, with the exception that a little more straw was used in packing. The bricks were not wrapped in paper, or otherwise protected, and the shipment of a number of cars demonstrated the precautions against breakage to be ample.

TESTS AS TO DURABILITY.

Enameled brick made by the above process have undergone two winters in a large light shaft in an office building and are just as good now as when laid in the wall. This is not very long, it is true, but there is every reason to believe that they will stand the test of time. As a severe test, about twenty of these brick were laid to form the top courses in a stack of one of the "baby" kilns. In this position they were subjected to red heat at one time, and to freezing at another. The brick remained in this position all winter and upon examination in the spring following, showed no signs of cracking, whatever.

A CONVENIENT PLANT.

The work outlined above was accomplished without building a special plant, by using what room was available and rigging up apparatus as necessity demanded in the progress of the work. The small kilns were all that was built especially. Realizing, however, that the process could be cheapened by better facilities for handling the ware, a build- was planned in which there were three rooms:

(a.) A glaze and engobe preparing room.

(b) A dipping room.

(c.) A drying room.

The arrangement of this plant is shown in the accompanying plates. There are two buildings under one roof, the

covered way between the buildings affording a good transfer in bad weather.

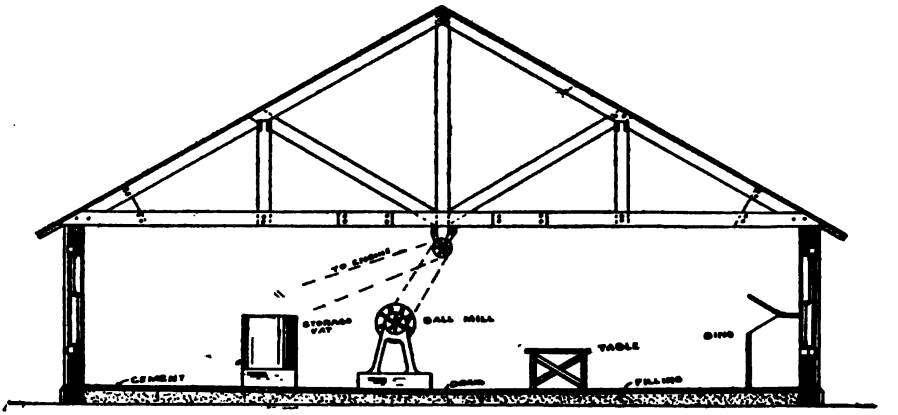
The structure is of brick, well lighted, and covered with a substantial slate roof. Cement floors are used, inclined to drains beneath each track so that the floors can be cleaned by flushing with a hose at any time. Cleanliness is all important in the enameling building. Dirt of any kind must not be allow to acccumulate.

For driving the slip apparatus (ball mill and blunger) a small gasoline engine would be preferable, on account of its cleanliness, and the fact that it is readily and economically worked periodically, for to get engobe and glaze for the capacity stated, the slip plant would run only on partial time.

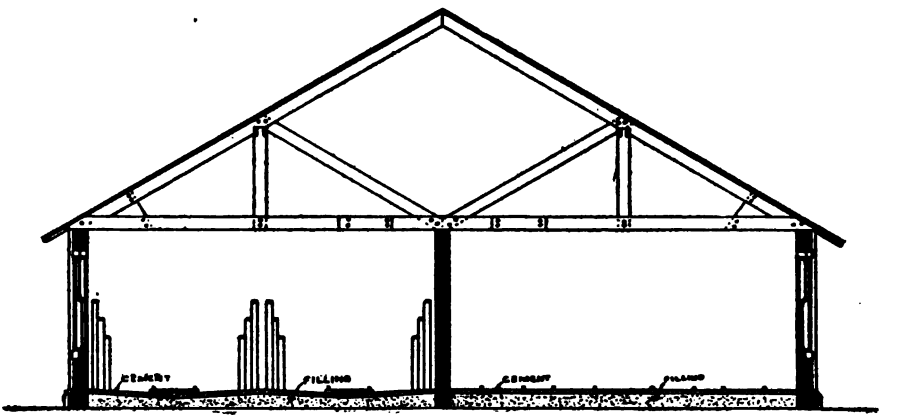
The biscuit brick are brought in on cars and placed on the long tripple benches from whence, having been enameled, they are taken and placed on pallets, loaded on cars and run into the drying room. The drying room could more properly be termed the "storage room" for there is no need to dry the bricks. Usually a great many were carried to the kiln right from the dipping bench, being dried in the 12 hours wood firing which started the burning. The main purpose of the drying room was to take care of the bricks dipped in the periods when there was no kiln room for them.

The kilns may be located near, preferably at the end opposite the slip-preparing room, so that the ware moves continuously forward. For kiln equipment, the writer favors square kilns of the Stewart type, in which the fire gases pass from the furnaces beneath the floor to bags on the opposite side. This type of kiln gives a very uniform distribution of heat.

The plant is designed for a very small capacity (4,000 brick daily) which may be increased, however, without much trouble. The given capacity is obtained by filling and emptying the benches twice daily, using one crew. To increase this to double or possibly triple, would merely mean filling and emptying the benches more often with several crews at work. The slip house is ample for three or four times the capacity stated, being designed to permit of enlargement.



SECTION ON B-B
SLIP ROOM



SECTION ON A-A
DIPPING AND DRYING ROOMS

PLANS FOR ENAMELED BRICK PLANT

DAILY CAPACITY 8000 STANDARD BRICK

SCALE, FT

The buildings shown in the above plates have not yet been constructed, but the process of enamelling is being carried on successfully on the original small scale as described above, at the works of the Columbus Brick & Terra Cotta Co., and the thanks of this society and the writer are due Mr. L. G. Kilbourne and Mr. Ellis Lovejoy, of this concern, for permission to publish this description of the work.

NOTE BY THE SECRETARY: This paper was read by title at the Boston meeting, and consequently received no discussion. Discussion is now specially invited, and if submitted in writing will appear in future volumes of these transactions.

THE GRINDING OF FLINT AND SPAR.*

BY

CHARLES F. BINNS, M. SC. ALFRED, N. Y.

(with 30 specimens and 5 micrograms.)

Scarcely a meeting of the Society has passed but some portion of this question has been introduced. It would seem as though there were a close agreement amongst us on two points, first, that our materials should be ground fine and, second, that this cannot be done.

The simple line of experiments discribed in this paper is an outcome of the thoughts expressed at previous meetings, and was undertaken with the view of throwing some light upon a vexed question.

Science can never be a partizan. It seeks truth and truth alone, and whether the truth be in accordance with our cherished beliefs or opposed to them, we want to find it.

The idea was to make up a series of bodies, similar in composition, by which the comparative action of coarse and fine materials should be illustrated.

SELECTION AND PREPARATION OF THE MATERIALS.

Kaolin.—The kaolin of the Hockessin Valley, Delaware, was chosen as a typical native kaolin.

The composition is:

Clay substance	90.42
Quartz	6.08
Feldspathic matter	3.50
	100.00

Ball Clay. — The brand selected is known as "Excelsior" and is sold by the Excelsior Ball Clay Company, Covington, Ky.

The composition is:

Clay substance	63.26
Quartz	35.00
Feldspathic matter	1.74
	100.00

*Read by title at Boston Meeting. Discussion specially invited.

Feldspar. — The ordinary spar of the Eureka Company, Trenton, N. J. was used.

The composition is:

Clay substance	4.28
Quartz	3.16
Feldspathic matter	92.56
	100.00

Flint.—The French Flint of the Eureka Company was assumed to be:

Quartz	100.00

It was washed successively through five lawns with the following results:

On 100 mesh, there was left nothing but a fragment or two of flint which had escaped grinding, and a small quantity of wood fiber from the linings of the cylinders, the flint having been ground dry in Alsing cylinders, lined with wooden blocks.

On 120 mesh was left	1.4%
On 140 " "	0.8%
On 150 " "	0.8%
On 160 " "	3.6%
Through 160 mesh	93.4%

Here let me say a word on the lawns used. Mr. Mayer added a note to the printed discussion of his paper published in the last Transactions, stating that there was a difference of standard as spoken of by different men. I want to say that he is right and I am wrong as reported.

The number of the lawn is the first two figures of the number of meshes to the linear inch, and the normal lawns in use in England are 14 and 16, No. 18 being only used for stain. This is as far as my experience goes.

I used 16 lawn as the finest in this experiment because, in my opinion, it will answer all the purpose. A lawn can never be an absolute test of fineness, its use is to separate unground particles, and not to gauge the size of the fine material.

A second sample of the same flint was taken and ground wet in a ball mill, until it seemed fine to the touch. This was also lawned through 16.

The residue of the unground flint left on each lawn

was now examined microscopically, and the grains measured. The sizes are expressed in the microscopist's unit "mu" (the Greek m) being of the value of one thousandth part of a millimeter. The size of the openings in the respective lawns is also given:

Grains left on	12 lawn measured		200 m			
" "	14	"	150 m	opening passed		140 m
" "	15	"	120 m	"	"	123 m
" "	16	"	100 m	"	"	105 m
Passed through	16	(not ground)	80 m	"	"	87 m
Passed through	16	(ground)	20-40 m	"	"	87 m

These figures are instructive. It would be expected that a close correspondence would be found between the size of the opening in a lawn and the grain which passed through it, but I scarcely hoped that the figures would fit together so neatly. The fact that the grains are sometimes larger than the holes, is due to the shape of each. The holes are square and the diagonals are longer than the measured diameter, also the grains are quite irregular in form.

A sample of pan-ground flint was then examined for the sake of comparison and the following results secured:

On 120 mesh	was left	3.6%
On 140	"	4.5%
On 150	"	4.9%
On 160	"	2.8%
Through 160 mesh		84.2%

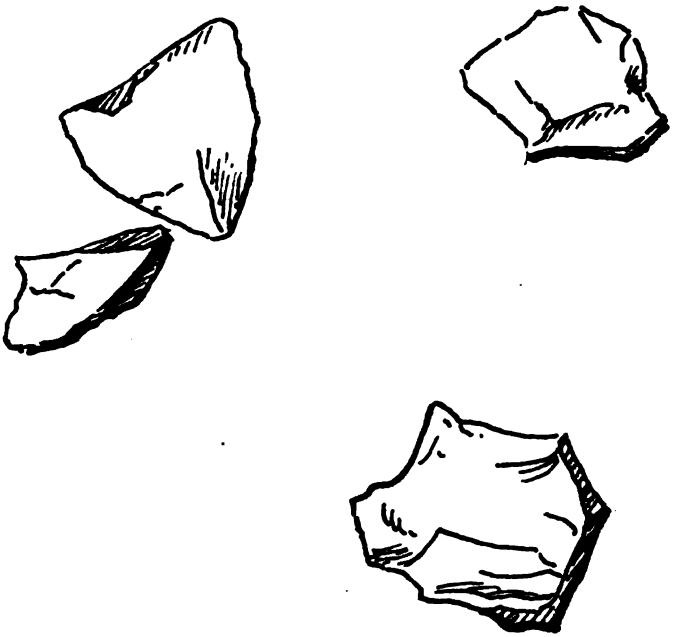
Thus as far as grinding is concerned, the sample of cylinder ground flint has the advantage, but of course this applies only to the actual samples handled.

Microscopic examination of the pan-flint reveals the fact that the grains are similar in shape to those of the cylinder ground. It has been argued, especially by English potters, that the grains of cylinder flint are globular, while those of pan-flint are angular. In the samples examined by me this is not the case. The micrograms shown in Plates I and II, prove that there is no appreciable difference in the grains of pan-flint, cylinder-flint and cylinder-spar.

Micrograms of the unground and ground flint are also shown, in order to illustrate the relative size of grain. Both

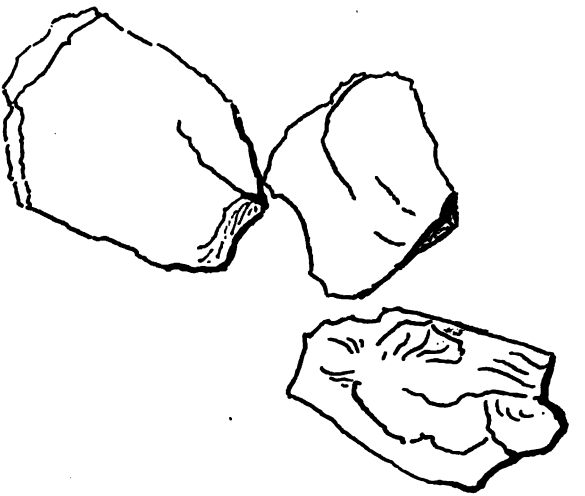
PLATE I.

MICROGRAM, SHOWING SHAPE OF GRAIN OF CYLINDER-GROUND FLINT.



MICROGRAM, SHOWING SHAPE OF GRAINS OF PAN-GROUND FLINT, OF SAME MAGNIFICATION.

PLATE II.



MICROGRAM SHOWING SHAPE OF GRAIN OF CYLINDER-GROUND FELDSPAR, SAME MAGNIFICATION AS FLINT.

have passed through 16 lawn but the second has been ground as discribed. (See Plate III.)

Both flints were dried after lawning and labelled "Sifted flint" and "Ground flint" respectively. Pan-flint was not used.

Feldspar: The normal body-spar of the Eureka Co. was treated in a similar manner.

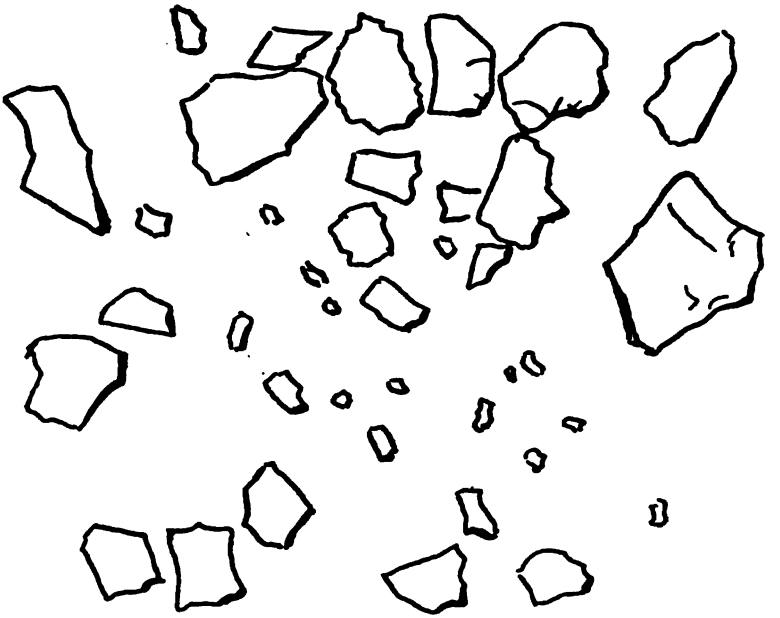
On 100 mesh	was left	3.6%
On 120	"	1.7%
On 140	"	2.7%
On 150	"	1.2%
On 160	"	2.2%
Through 160 mesh		88.6 %

A second sample was then ground fine. This, under the microscope, was found to be almost identical in size of grain with the ground flint.

MIXING THE BODIES.

It was decided to maintain the clays at a constant ratio because the experiment was only concerned with flint and spar.

PLATE III.



MICROGRAM, SHOWING DRY-GROUND CYLINDER FLINT, WHICH HAD BEEN PUT THROUGH 160 MESH LAWN. MAGNIFIED 250 DIAMETERS.

MICROGRAM, SHOWING SAME FLINT AFTER GRINDING IN WET BALL MILL, AND PUTTING THROUGH 160 MESH LAWN. MAGNIFIED 250 DIAMETERS.

Four series were projected which should contain respectively:

I Sifted flint and normal spar
II Ground flint and normal spar
III Sifted flint and ground spar
IV Ground flint and ground spar

and each series was to consist of three members containing:

No. 1	Flint	40	Spar	10
No. 2	"	30	"	20
No. 3	"	20	"	30

in accordance with the table below:

SERIES I.

No.	Kaolin.	Ball Clay.	Sifted Flint.	Normal Spar.
1	20	30	40	10
2	20	30	30	20
3	20	30	20	30

SERIES II.

No.	Kaolin.	Ball Clay.	Ground Flint.	Normal Spar.
1	20	30	40	10
2	20	30	30	20
3	20	30	20	30

SERIES III.

No.	Kaolin.	Ball Clay.	Sifted Flint.	Ground Spar.
1	20	30	40	10
2	20	30	30	20
3	20	30	20	30

SERIES IV.

No.	Kaolin.	Ball Clay.	Ground Flint.	Ground Spar.
1	20	30	40	10
2	20	30	30	20
3	20	30	20	30

The rational composition of each series is identical, and and works out as follows:

	Clay Substance.	Quartz.	Feldspathic Matter.	Total.
No. 1—	37.3	52.1	10.6	100
No. 2—	37.8	42.4	19.8	100
No. 3—	38.3	32.4	29.3	100

The whole of the twelve bodies were weighed up, blunged, and lawned through No. 10 to break up the clay and secure a perfect mix. A portion of each slip was used to cast a small vase, and the remainder was made into clay and pressed into plaster molds to form small slabs, thus showing the structure of both cast and pressed ware.

In order to throw light upon the mutual reactions of clay, flint and spar, two more series were prepared:

SERIES V.

No.	Kaolin.	Ball Clay.	Sifted Flint.	Ground Flint.	Normal Spar.
1	..	..	50	..	50
2	..	..	..	50	50
3	20	30	..	..	50

SERIES VI.

No.	Kaolin.	Ball Clay.	Sifted Flint.	Ground Flint.	Ground Spar.
1	..	..	50	..	50
2	..	..	..	50	50
3	20	30	..	..	50

In these six trials, the relative action of spar in each condition upon flint in each condition and upon the normal clay blend is illustrated.

The mixtures of spar and flint were worked with mucilage to provide an artificial plasticity, those of spar and clay were treated as before described. The whole were made up into small plaques, and, with the members of the first four series, placed in the same sagger with the cones exhibited and fired to cone 9. The firing was maintained (natural gas being the fuel) during 14 hours in an oxidizing atmosphere. The cooling took 26 hours.

The whole of the bodies were vitrified and the other features presented are tabulated below:

SERIES I.

No.	Texture.	Color.	Ink Test.
1	Granular	Cream	Slowly absorbed
2	Granular	Stony	Slightly retained
3	Granular	Stony	Slightly retained

SERIES II.

1	Dense	Cream	Slightly absorbed
2	Porcellanous	Stony	No absorption
3	Porcellanous	Stony	No absorption

SERIES III.

1	Granular	Cream	Slightly absorbed
2	Granular	Stony	Slightly retained
3	Granular	Stony	Slightly retained

SERIES IV.

1	Dense	Cream	Slight absorption
2	Porcellanous	Stony	No absorption
3	Porcellanous	Stony	No absorption

The "ink test" consists in drawing a pen full of ink across a broken surface. Some times the ink is drawn into the pores of the body, some times it spreads through the irregularities of the granular surface, and some times it remains where placed and can be washed off without a stain. The test is not exactly scientific, but it is one with which all potters are familiar and is, moreover, used as a decisive test of quality by some buyers. The comparison of the trials for determining the effect of grinding will lie between Series I, where both materials are crude; Series II, where the flint alone is ground; and Series III, where the spar alone is ground.

In Series I, numbers 2 and 3, a granular structure is observed, with complete vitrification, but the coarse grains allow the ink to spread. There are also numerous brilliant specks due to fragments of spar. These specks disappear in Series III, but the structure appears otherwise unchanged.

In Series II, however, an important alteration is noticeable. Here the flint has been ground but not the spar. The granular fracture is lost, and in its place there is a fine porcelain texture upon which the ink has no effect whatever. This is maintained in Series IV, the only difference being that the bright specks before alluded to disappear.

The evidence so far is that the grinding of flint has a very marked effect, but that the grinding of spar has less. This seems to be the case, whether flint or spar is in excess. Body No. II-2 with 30 per cent. ground flint and 20 per cent. normal spar, is as vitreous as No. III-3 which has 20 per cent. sifted flint and 30 per cent. ground spar. The argument from this is that if the flint be fine, the influence of the spar will be increased, whether it be fine itself or not.

Turning now to the later series, V and VI, some interesting evidence is found. The relative action of ground flint and ground spar is in accordance with that stated above, but the action of the spar on clay is most instructive. The value of ground spar is shown better here. It becomes much more powerful by grinding. On comparing VI-3 with V-3 we notice the higher vitrification of the former.

The blends of spar and clay alone have the appearance of a frit or slag. They seem to be not translucent, but transparent, and to this fact is due their dark color. The mixtures of spar and flint seem not less vitreous or translucent, but they lack the glass-like transparency which the clay mixtures possess. From this argument may be deduced that clay substance, perhaps because already in combination, dissolves in the fused spar, while the flint, being free silica, remains in suspension.

A new light is thus cast on the problem of color and translucency. Spar appears to render the clay mass transparent and dark, because the rays of light penetrate and are not reflected. The effect is similar to that of a depth of water; the color becomes dark by the absorption of light, even tho the water itself be colorless. But now, if a sudden agitation of the water takes place, and numerous air bubbles be formed, the whole becomes a pearly white, translucent still, but the darkness is lost. So when in the fused mass of spar

there are present a large number of flint fragments, the reflection from these imparts whiteness, and through the spaces between them the light rays can pass. Add to this the fact that most of the flint grains are in themselves translucent, and there is an explanation of the combined whiteness and translucency in the presence of flint, which does not occur in its absence. Obviously then the fine grained flint will have a stronger influence, for there are just so many more points for the reflection of the light rays.

The grinding of spar has the effect of increasing its power at a given temperature. The fragments of spar, rendered fluid by heat may be likened to a number of pools of liquid, charged with the duty of joining with each other over an intervening space of solid ground. If these pools be separated into smaller portions, their size being one-fourth and their number four times as great, it is obvious that the solid ground will be covered so much more thickly and the distance between the pools correspondingly reduced. The effort to join each other will not be so great, and energy will be economised.

Now, since this energy is in the form of heat, and fuel costs money, it is certain that a reduction in the size and an increase in the number of spar particles means a saving. In addition to this, the reduction in the size of the liquid points means a more uniform mass. There is less danger of blistering from unequal distribution of flux, and there is an economy of material.

It may be argued by some that finer grinding of flint and spar means more shrinkage, and a consequent expense, in some cases, for new molds. But it will be found that, with the materials properly ground, it is possible to increase the flint and lower the spar, thus helping to maintain the size and affording a whiter body.

The evidence of this experiment is strongly in favor of fine grinding, and the time will surely come, and that quickly, when the progressive potter will formulate a demand in this direction which will admit of no denial.

DISCUSSION BY LAWRENCE E. BARRINGER,*
SCHENECTADY, N. Y.

On the subject of fine grinding of flint and feldspar I should like to have Mr. Binns asked whether he has noted any change in the chemical behavior of feldspar after long, continued grinding. Of course, in the case of flint, fine grinding is not accompanied with any chemical change, but geologists in studying rock decomposition have found that natural silicates may be decomposed by grinding with cold water. Daubree found on rotating angular fragments of feldspar in an iron barrel with water at a velocity of one metre per second, that they gave .003 of their weight of clay *per kilometer*. Further, he took *three* kilograms of orthoclase and rolled in an iron barrel for 192 hours (460 kilometers) and found that it gave 2.7 kilos of clay and that 12.6 grams of K_2O were set free as silicate. Distilled water was used. The experiment proved that feldspar may be decomposed by water alone and even by cold water, forming kaolin and alkaline silicate. M. Heim, in investigating the settling of pottery glazes, also found that long continued grinding of frits resulted in the separation of free alkali, which caused the glaze to set hard in the bottom of the mill. I am interested, therefore, in knowing whether it is possible to seriously affect feldspar from a potter's standpoint by wet grinding it for long periods.

*Communication sent in to the Secretary.

NOTE ON FINE GRINDING OF GLAZES.

BY

OWEN CARTER, POOLE, ENGLAND.

I notice in a paper on page 64 of Vol. I, of the Transactions, the writer says:

"All these glazes could not be used in commercial work as they carry too much frit. They settle too quickly, and are very irregular in dipping."

The experience my firm have had on this subject may be of interest, if not of use to some members.

The English potters have been for some time fearing the passing of some kind of legislation prohibiting the use of glazes of high solubility as regards lead.

The difficulty was that if one made a frit to meet the proposed government requirements, an 07 or 08 fire was necessary (our usual heat). It was, therefore, very evident that no raw flotative ingredients could be added, raw lead being forbidden, and soft alkaline frits being impractical for various reasons.

The only thing to be done was to use the pure frit, and then came the difficulty of settling; etc. If the frit was ground wet in the usual way, fine enough to keep in suspension in water for dipping, the glaze on the surface of the tiles, when the water had dried out, would become covered with fine cracks, and upon firing, the glaze would peel up and collect into beads or patches on the surface leaving bare places. Thin dipping, such as is practiced with earthenware, would prevent this, but colored glazes on tiles must be dipped thickly.

The solution of the problem was found in grinding the frit dry in Alsing cylinders. I only recollect one case of peeling up with a dry ground glaze. It seems almost impossible to dry-grind it too finely. I do not say that too fine grinding is in every case the reason of glazes peeling and

gathering into patches and beads; but I think that this is usually the cause. Fortunately it can generally be noticed before the tiles are fired, if one is on the look-out for it, as after dipping and standing a little time and drying, the surface of the tile will be noticed covered with these very fine cracks, especially where the glaze is thick.

The firm I am connected with being anxious to do away with the use of raw lead in their glazes, use a frit of low solubility wherever possible. Probably two thirds of the total glaze used at their works is just the pure frit, without the addition of any raw material. It is, however, impossible to use it for all the colors.

THE EFFECT OF FINE GRINDING IN THE MANUFACTURE OF BEDFORD SHALE BY THE DRY AND PLASTIC PROCESSES.

BY

W. G. WORCESTER, PARKERSBURG, W. VA.

In the economic value of the Ohio clay deposits, the red Bedford Shale plays an important role. This is a clay of peculiar and striking characteristics, valuable to the manufacturer for several reasons: First of all, it produces on burning one of the most magnificent red colors known, probably the finest red color of any clay in the United States. In addition to this, it possesses the property of vitrifying at a slow and safe rate, since its point of vitrification and its melting points are separated by a wide interval. Further, it retains its fine red color even when vitrified, being the only clay known to the writer which does so. This red color owes its source to the presence of finely divided ferric oxide, already indicated by the chocolate color of the shale. In texture, the shale is extremely fine grained, and is readily made up to a plastic body. In addition, the bed appears to be remarkable in uniformity of composition and character.

These properties are, however, counter-balanced to some extent by the comparatively large shrinkage, and the fact that its fine and dense body renders it hard to burn, owing to the internal gases at the vitrifying point being unable to escape, thus causing bloated or blistered ware unless burnt slowly.

It is now the purpose of this paper to show how the characteristics of this clay, its magnificent red color, and its property of retaining this color at vitrification, may be retained and its disadvantages eliminated, making this elimination complete and increasing the safe burning of the clay.

It has not been known until quite recently, outside of the circles of geologists, that red Bedford Shale is found at more than one point in the state, that being at and near South

Park. However, it is now known to be found at various points from the lake shore to within a short distance of the Ohio River. As before said, it was found at South Park, and near Cleveland; at Elyria, there are beds from twenty to thirty feet thick. The Vermillion River received its name from the fact that its banks near Brownhelm are composed of beds of the red Bedford Shale over forty feet thick, and the waters of the river washing this red deposit are colored red, hence the name Vermillion. Passing south in the state it is again reported at Leesville, Crawford County, next, at Galena and Sunbury, Delaware County. And from the latter place south, it has been traced uninterruptedly for a distance of fifteen miles by the writer. A typical section on this line is about as follows:

Berea Grit	0 to 30 feet.
Chocolate Shale (Bedford).....	10 to 40 feet.
Green or Olive Shale (Bedford)..	0 to 20 feet.
Black Shale (Huron).....	(Unknown depth.)

At Taylors's Station there is a fine deposit that is to be worked this year for the first time. It was from this deposit that the samples for these tests were taken. Passing on south of Taylor's Station it again crops out near Marcy, Fairfield County. Then further south large deposits of it are found at Waverly and Piketon; though as it passes south, the red chocolate color gradually disappears and is replaced by a blue or gray colored material. So it can plainly be seen that this deposit is more widely distributed than has heretofore been known by clay workers. It has only been worked at one point, Independence, Ohio, being there made into the famous Akron Red pressed brick, a color which it was thought could not be reproduced elsewhere.

The practical question now arising is that of the reduction of shrinkage by some non-plastic material. The following "leaning" or non-plastic substances are available under ordinary conditions; first, the use of grog from the same clay, which will reduce the drying but not the burning shrinkage; second, the use of fire-clay grog, which will reduce both the drying and burning shrinkage, but will practically spoil the color; third, the use of sand, which will do away with shrinkage, but is very apt to affect the color materi-

ally, and is liable to introduce undesirable impurities owing to its varying composition. Hence, none of these three are perfectly satisfactory.

I have discovered the fact that the Berea Grit, a fine-grained sandstone of uniform composition which invariably accompanies the Bedford Shale, assumes on burning a reddish color. This suggested the use of this stone as a non-plastic material most suitable for reducing the shrinkage of the shale. No economic objection can be offered to this argument, and there only remained to prove by actual experiment its influence upon the color, shrinkage and texture of the burned shale.

The plan of the experiments carried out is as follows:

First. It was necessary to determine the shrinkage of the shale by itself, and at the same time to inquire into the effect of grinding the shale coarser or finer. I also found it necessary to determine the properties of the shale shown in working by the plastic and dry-press processes. I therefore prepared the all-clay series as follows:

No. 1—All clay, over 100 mesh.
No. 2—80 per cent. No. 1, 20 per cent. clay, 8 to 20 mesh.
No. 3—80 per cent. No. 1, 20 per cent. clay, 20 to 40 mesh.
No. 4—80 per cent. No. 1, 20 per cent. clay, 40 to 60 mesh.
No. 5—60 per cent. No. 1, 40 per cent. clay, 8 to 20 mesh.
No. 6—60 per cent. No. 1, 40 per cent. clay, 20 to 40 mesh.
No. 7—60 per cent. No. 1, 40 per cent. clay, 40 to 60 mesh.
No. 8—40 per cent. No. 1, 60 per cent. clay, 8 to 20 mesh.
No. 9—40 per cent. No. 1, 60 per cent. clay, 20 to 40 mesh.
No. 10—40 per cent. No. 1, 60 per cent. clay, 40 to 60 mesh.

Second. It was also necessary to prepare shale mixtures in which the crushed Berea grit is incorporated, analagous to the mixtures of the all clay series; for this purpose, the sandstone was crushed and screened similarly to the shale, and the following mixtures prepared:

No. 1—All clay, over 100 mesh.
No. 02—80 per cent. No. 1, 20 per cent. stone, 8 to 20 mesh.
No. 03—80 per cent. No. 1, 20 per cent. stone, 20 to 40 mesh.
No. 04—80 per cent. No. 1, 20 per cent. stone, 40 to 60 mesh.
No. 05—60 per cent. No. 1, 40 per cent. stone, 8 to 20 mesh.
No. 06—60 per cent. No. 1, 40 per cent. stone, 20 to 40 mesh.
No. 07—60 per cent. No. 1, 40 per cent. stone, 40 to 60 mesh.
No. 08—40 per cent. No. 1, 60 per cent. stone, 8 to 20 mesh.
No. 09—40 per cent. No. 1, 60 per cent. stone, 20 to 40 mesh.
No. 010—40 per cent. No. 1, 60 per cent. stone, 40 to 60 mesh.
No. 011—70 per cent. clay, 30 per cent. stone, ground in ball mill three hours.

The shale was put through a small jaw crusher which reduced it until it passed through an eight mesh screen; it was next thrown onto a twenty mesh screen, and carefully sifted; all that remained on the twenty mesh was saved; the part that has passed the twenty mesh was put onto a forty mesh and sifted again; what remained on the forty mesh was put aside. The portion which was finer than the forty was sifted on a sixty mesh, and the part that did not pass through it was saved. The balance of the material was then sifted on the hundred mesh screen, and that which passed through it was taken as the starting point for all of the tests.

The various mixtures were then weighed out and mixed by passing them through sieves and repeated stirring. For the dry press tests, the different batches were placed in a clean enameled pan, one at a time, and mixed with water until each seemed to have the proper amount for dry pressing. As soon as each was supplied with its requisite amount of moisture, it was placed in a fruit can, screwing the top down tight and allowing it to stand for forty-eight hours, so as to distribute the moisture evenly throughout the mass.

For the plastic tests, the mixtures were taken one at a time in a clean basin, and water added until they could be worked nicely by the hands; it was then worked well to free it from air as far as possible, and was then molded at once into small brickettes, using brass molds such as are used for making cement brickettes for tensile strength tests. As soon as removed from the mold, they were each in turn weighed very accurately, and shrinkage marks made upon them by means of a very thin spatula. The brickettes were then placed upon smooth boards, and allowed to dry slowly until leather hard; they were then put upon racks above a hot plate, and the heat was slowly increased until they were to all appearances dry. Taking them from the racks they were placed in small drying ovens, and kept at 120 degrees C for three hours; removing them from the oven while hot, they were placed in dessicators until cool, and then weighed and also measured, to note the drying shrinkages and percentage of water used for mixing.

The work of making the dry press tiles was done as follows: The mixtures which had been placed in the fruit cans were taken to a testing machine used for making crushing and cross-breaking tests in the mechanical department the of Ohio State University. A die two inches by two inches was used, and the machine set to give 12,000 pounds pressure per square inch; as it was desired to have every tile receive the same pressure, and knowing that anything like accurate results could not be obtained by trying to guess at the amount of pressure applied to each. As soon as removed from the die, they were numbered, and a mark made with a knife from side to side from which the shrinkage was measured. These tiles were also weighed at once, and were dried in the same manner as the plastic, except that they were placed over the hot plate immediately. After allowing them to cool in the dessicator, they were weighed again and each tile was measured for shrinkage, but the shrinkage, if any, had been so slight that it was impossible to measure it.

PER CENT. OF WATER.

All-Clay Series.			Stone Series.		
No.	Dry Press.	Plastic.	No.	Dry Press.	Plastic.
1	5.2%	20%	1	5.2%	20%
2	4.5	11	02	6.2	24
3	6.3	20	03	6.6	19
4	4.8	20	04	9.3	15
5	6.7	17	05	5.7	16
6	9.3	19	06	1.4	18
7	9.3	19	07	7.8	18
8	7.3	18	08	3.9	13
9	8.4	12	09	6.6	16
10	7.5	19	010	7.8	15
			011	6.6	23

BURNING.

In the carrying out of this part of the investigation, a small rectangular down-draft kiln at the ceramic department of the Ohio State University was used, The tile and brickettes were packed in a sagger rather closely, though using clean white sand between each. The sagger was then placed as near the center of the kiln both vertically and horizontally as possible; bricks were then set around the sides

and over the top of the sagger in order to hold the heat as even as possible. From a previous trial-burn, it was known that cone 06 would bring this clay to a good sound body, though not vitrified, so for this burn cones 07, 06 and 05 were placed immediately in front of the sagger and in the sagger. The firing was proceeded with in the usual way using soft coal and gas coke for the fuel. At the end of twenty-eight hours, cone 07 was nearly down and 06 started, so the fire was allowed to burn clear and the damper lowered; the kiln was then allowed to cool twenty-four hours, after which time the ware could be handled.

The various trials were then measured again, and each one weighed; they were immersed in distilled water for a period of forty-eight hours; they were then taken out one at a time, the surface water wiped off with a clean towel, and weighed at once. This gives the weight of water absorbed as a measure of porosity.

RESULTS OF THE BURN.

All Clay Series, Dry Press.

In selecting the most suitable mixture, the points taken into consideration were: first, color; second, shrinkage; third, porosity; fourth, indications of proper burning. In the accompanying table all the data obtained are collected; it will be seen that the densest mixtures show black cores, indicating difficulty of burning which must rule out these mixtures. With coarser grains, the difficulties of burning disappear and at the same time neither the burning shrinkage nor porosity are increased. The mixtures selected are first number 3, whose composition is 80 per cent. clay over 100 mesh, 20 per cent. clay 20 to 40 mesh and which when made up with 6.33 per cent. water will give 4.43 per cent. burning shrinkage and 3.48 per cent. porosity. The second best is number 10 whose composition is 40 per cent. clay over 100 mesh, and 60 per cent. clay 40 to 60 mesh, and when made up with 6.63 per cent. water will give 3.13 per cent. burning shrinkage and 7.74 per cent. porosity.

No.	Appearance.	Remarks.
1	Black Core nearly throughout.	Rejected on acc't of Black Core.
2	Black Core 4-5 cross section.	Rejected on acc't of Black Core.
3	No Black Core.	Accepted. (Best.)
4	Black Core ½ cross section.	Rejected. Black Core.
5	Black Core ¾ cross section.	Rejected. Black Core.
6	No Black Core.	Fifth best.
7	No Black Core.	Fourth best.
8	Signs of Black Core.	Rejected. Too coarse.
9	Signs of Black Core.	Third best.
10	Color exceptionally good.	Second best.

All Clay Series, Plastic.

The selections of the most suitable mixtures in this series was rendered somewhat difficult by the fact that they were under-burned, so that the proper color selection could not be made. Black cores were only shown by two mixtures, Nos. 1 and 2. All that could be done in this series was to judge the color from the corresponding colors of the dry.press series which were burned harder, and to select the mixture showing the lowest shrinkage and porosity. The most favorable mixtures are Nos. 7 and 8. Number 7, whose composition is 60 per cent. clay over 100 mesh and 40 per cent. clay 40 to 60 mesh with 19 per cent. water, gives 9.37 per cent. total shrinkage and 6.8 per cent. porosity. The second best, number 8, whose composition is 40 per cent. clay over 100 mesh, and 60 per cent. clay 8 to 20 mesh, when mixed with 18 per cent. water gives 8.6 per cent. total shrinkage and 7.66 per cent. porosity. The data are shown in the following table.

No.	Appearance.	Remarks.	Drying Shrinkage.	Burning Shrinkage.	Total Shrinkage.	Porosity.
1	Good color 1-3black	Rejected	6.80	5.70	12.50	8.11
2	Good color ½ black	Rejected	6.25	4.69	10.94	6.10
3	Soft burned		6.25	3.91	10.16	7.27
4	Soft burned		7.81	4.70	12.50	5.79
5	Soft burned	2nd best	5.86	3.13	8.99	7.90
6	Soft burned	3rd best	6.25	3.91	10.16	7.24
7	Soft burned	1st best	5.47	3.90	9.37	6.80
8	Soft burned		5.47	3.12	8.59	7.66
9	Soft burned		7.42	2.74	10.16	7.28
10	Soft burned		6.25	4.69	10.94	5.63

Stone Series, Dry Press.

Numbers 02, 05, 06, 08 and 09 were rejected on account of either pale color, or being rough on the surface due to sand grains. The most favorable mixtures are 03 and 011, the latter however, having the defect of showing some lamination. The cause, however, is not fully determined, as it may be due to the high pressure, or due to too much or too little water, or too great fineness of grain. Number 03 when made up with 6.6 per cent. water, showed 2.4 per cent. total shrinkage with 7.15 per cent. porosity. Number 011 when made up with 6.63 per cent. water, showed 3.13 per cent. total shrinkage and 7.74 per cent. porosity. The data are not published in full, but the four best mixtures are selected for the following table.

No.	Remarks.	Drying shrinkage.	Burning shrinkage.	Total shrinkage.	Porosity.
03	Selected, as the best of the Dry-press Series.	None	2.42		7.15
04		None	2.28		17.71
07		None	1.72		10.95
011		None	3.13		7.74

Stone Series, Plastic.

None of the mixtures in these series showed any black cores whatever; they suffered the defect of not having been burned enough. Number 011, having for its composition 70 per cent. clay, 30 per cent. stone, and having been ground in a ball mill for three hours, showed defect of being laminated quite strikingly, which suggests that it is too fine. Numbers 04 and 07 were the most suitable mixtures, 04 with 15 per cent. water shows 8.6 per cent. total shrinkage and 8.8 per cent. porosity. Number 07 with 18 per cent. water showed 7.42 per cent total shrinkage and 10.55 per cent. porosity. These porosities can only be considered as relative values, since when burned to the proper temperature they would show a much smaller percentage. But it is evident that any gain in decreased shrinkage is to some extent balanced by the increased porosity. The same four mix-

tures are here also the best, and are shown in the table below.

No.	Remarks.	Drying Shrinkage.	Burning Shrinkage.	Total Shrinkage.	Porosity.
03	Selected, as the best of the Plastic Series.	2.34	5.47	7.81	11.26
04		4.69	3.90	8.59	8.84
07		5.47	1.95	7.42	10.55
011		6.25	3.91	10.16	10.77

GENERAL CONCLUSIONS.

A remarkable fact noticed in this test was that both of the dry press series, though burned with the plastic trials showed a better color and greater hardness. From the results before us, the conclusion to be reached would be that where dry pressing is practiced, no addition of sand is necessary. It might pay to use a small proportion of 60 to 100 mesh stone, say about 10 per cent, as this will facilitate the proper burning of the ware by opening up its pore system to a small degree, and thus tend to prevent bloating. However, greater care should be taken to properly size the grains of clay than is now being done in most brick works. For the red Bedford Shale, the most suitable combination of sizes is shown by Number 3, followed by Number 10.

On the other hand, where it is necessary to work the red Bedford Shale by the plastic process, as for roofing tile, paving brick, stiff-mud face brick, machine-made terra cotta, etc., ground stone mixture should be used. The most suitable mixtures are the compositions of numbers 04 and 07; in addition to decreased shrinkage, another great advantage is the increased safety of burning, which will shorten the time and decrease the loss of ware due to black cores and will soften it considerably. These advantages, therefore, greatly overbalance the somewhat greater porosity.

One great advantage of the plastic stone series is that over-burning cannot occur. Even in careless burning, bloating is made almost impossible. This last statement was verified by myself, in an experiment in which I made cones of

both the *all clay* and *stone series*, burning them to a temperature of 1,300°C, in a reducing fire. The all clay cones were badly bloated and blistered and had melted down, while on the other hand, the sand series though vitrified, had either commenced to fuse smoothly, without bloating, or were still in an unfused condition.

The cone tests showed very consistently a very peculiar fact which was unexpected to me; this was that the mixtures highest in stone, 60 per cent, were the most fusible ones, which is contrary to the laws of fusibility applied to ferruginous clays, but in accordance with the laws referring to the refractory clays. One explanation which suggests itself, is that iron of the clay may have been drawn into reaction with the silica, forming a fusible iron silicate which started the fluxing action, and caused the cones to fuse earliest.

ON THE PRODUCTION OF AN EASILY FUSIBLE GLASS WITHOUT THE USE OF LEAD OR BORACIC-ACID.

BY
PROFESSOR EDWARD ORTON, JR., E. M.,
COLUMBUS, OHIO.

I. Purpose of the Investigation.

In the last two meetings of this Society, papers have been read and discussions have been had on certain defects of the Seger cone as a means of control for the burning of clay wares. The criticisms raised have been directed almost entirely to that part of the cone series devised by Dr. E. Cremer of Berlin, which was grafted onto the original series developed by Dr. Seger, some time after the latter had been brought to public attention, and also to the lower three members of Seger's series, in which Fe_2O_3 is used as a flux. These criticisms have not been directed to the Seger cones free from iron, nor to the recent series devised by Dr. Hecht for the measurement of temperatures below 950°C.

The various charges which have been made and substantiated against these ferrugineous cone mixtures are as follows:

1st. It has been shown by Gorton[1] that the presence of ferric oxide as a flux constitutes a source of irregularity in their heat reactions, from the fact that Fe_2O_3 is easily reducible to 2 FeO, by CO or CH_4 or H in the kiln gases, and that FeO is known to cause fusion in a silicate mixture at temperatures much lower than Fe_2O_3 will do. Wheeler gives the difference as amounting to 200 deg.C, and while the writer is unable to find other support for this statement, his own experience confirms it, at least so far as agreeing that the difference is a considerable one. A silicate which is designed to fuse uniformly at one temperature, should at least avoid the use of fluxes which can, by mere variations

1 Trans. American Ceramic Society, Vol. II, pages 60-64.

in the firing, develop considerable difference in fusing point.

2nd. The use of a boro-silicate frit as the basis of the Cremer series has been condemned. It has been pointed out by Hottinger[1] that boracic acid is a very volatile constituent, which not only escapes by volatilization, but is easily set free from its compounds by SO_3 in the kiln gases, and thus expelled much more rapidly than would be the case, if it were heated in pure air alone. This fact has been used to explain the "drying out" of the tips of the cones; the formation of a hard shell, and a molten interior; the failure to bend at the proper time, and other defects of the Cremer cones.

3rd. The formation of $CaSO_4$ from the $CaCO_3$ used in the cone series, or from the CaO formed by the breaking down of the latter, has also been pointed out as a fruitful cause of trouble, especially in the low temperature cones. In firing kilns at low temperatures, the kiln gases are generally maintained in oxydising condition, and thus no opportunity occurs to expel the SO_3 by reduction to SO_2, which generally does take place, intentionally or otherwise, when the kiln is to reach a temperature above cone 1. The CaO which goes into combination with SO_3 is supposed to be withdrawn from duty as an active flux, until the acid is finally expelled. Meanwhile, the fusion of the cone is supposed to be delayed for the lack of the requisite quantity of effective flux.

All three of these charges are based upon numerous authenticated cases, in which it has seemed clear that the reason assigned has been undoubtedly to blame for the trouble. For instance, Gorton showed that by omitting iron from his cones, that he greatly improved their behavior. Hottinger showed that cones and glazes of the same composition, fired in the open kiln were defective as described, while fired in saggers washed with lime to intercept the SO_3, the same cones and glazes behaved perfectly. Lastly, the writer has found that cones which failed to fuse at the proper temperature, and which swelled badly on fusion, con-

1 Trans. American Ceramic Society, Vol. II, page 65—Vol. III, page 180.

tained 2.5 per cent. of sulphur which was not in the cone when placed in the kiln, and which had undoubtedly been absorbed from the gases during firing, and which undoubtedly prevented normal fusion.

The writer has undertaken the following investigation with a view to ultimately producing a series of cones which will occupy the temperature range now covered by the Cremer series, but which will avoid wholly or in part the above defects. The principal points in which it seems that such an improved series should differ from that of Dr. Cremer, are:

1st. The omission of iron oxide as a flux, and its substitution by some other oxide not subject to easy reduction.

2nd. The replacement of the boro-silicate frit by one less volatile, and less easily attacked by acid kiln gases.

3rd. The cessation of the use of lime and other strong bases in the form of carbonates or oxides, which invite the formation of sulphates during the early portion of the burn.

II. General Method of Attack.

It is obvious at the outset, that these limitations threaten to make the task a somewhat difficult one. There remains commercially available for use only the following oxides, capable of making colorless or white silicates:

$$\left.\begin{array}{l} K_2O \\ Na_2O \\ CaO \\ MgO \\ BaO \\ ZnO \end{array}\right\} Al_2O_3, SiO_2$$

with the possible addition of P_2O_5, as an available acid. These substances may be compounded into a frit or may be used raw in any form which will gain the end in view.

But little experience is required in the compounding of raw stoneware glazes, to know that with this preceding list of availables, no raw or unfritted mixture fusing at the melting point of cone 010, or 950 deg.C can be made. In fact, excluding BaO, no glaze can be made from the others which fuses below cone 1, without resorting to other sources of alkali than feldspar, which is impractical in cone manufacture. The use of BaO has not been extensively tried by the writer,

but the results where tried were such as to indicate that it would not greatly reduce the point of initial combination, though quite fusible after combination had been once perfected. Hence, fritting must be accepted at the outset as absolutely essential to any successful effort.

Dr. Hecht has pointed out[1] that the proper compounding of a low temperature cone mixture requires a frit or a fusible glass base, and a certain amount of raw or unfritted materials. The frit cannot properly exceed a certain proportion of the total mixture, because glasses soften and pass from rigid solids into liquids so rapidly that cones made largely from them would be too sensitive, and would not undergo a gradual vitrification and gentle fusion. The raw ingredients play an important part in holding up the body mass, and requiring an appreciable time to complete their vitrification and fusion, even after the glass has fused completely.

III. Attempt to Use Common Glass.

The first experiment made was to use common lime-soda glass as the frit, as it can be had cheaply and abundantly.

Its average composition may be taken as

$$\left.\begin{matrix}0.5\ Na_2O\\0.5\ CaO\end{matrix}\right\}2.50\ SiO_2$$

Accordingly, a lot of scrap glass was procured, and ground in a wet ball-mill, till it readily passed through a 150 mesh screen. This was dried and weighed as a fine powder. The formula of the first test mixture was roughly:

$$1\text{-A}-\left.\begin{matrix}0.3\ Na_2O\\0.7\ CaO\end{matrix}\right\}0.3\ Al_2O_3,\ 2.10\ SiO_2$$

The alkali, 0.3 eqv. of the lime, and 1.50 eqv. of the silica were derived from the glass, while 0.4 eqv. lime was supplied as bone ash, under the hope that the latter might be used as a source of lime, which would not open up the troubles incident to the use of $CaCO_3$ in its commercial forms. The alumina was supplied as kaolin, so that the batch weights of the mixture were:

	Powdered window glass..	41.8
1-A —	Calcined bone.....	13.7
	Kaolin	25.8
		81.3

1 Thon Industrie Zeitung, Vol.—, page 189.

These ingredients were ground intimately together, dried, mixed with dextrine paste, and molded into test cones, which were then tested against the regular cones from 010 to 1.

The glass-bone-kaolin mixture fused a little later than cone 1, probably at about cone 2; no cone number 2 was put in the test, as the temperature required for fusion of the unknown mixture was a complete surprise. Even when it went down, it made merely a nice white porcelain, and indicated that it was some distance from liquidity.

This single test at once dispelled any hopes of being able to utilize bone as a vigorous aid in low temperature work. It did not shatter our hopes of being able to still utilize glass alone, which we next prepared as follows:

TABLE I.

No.	Ingredients	Molecular Formula			
		Na_2O	CaO	Al_2O_3	SiO_2
1-B	Glass alone	0.5	0.5	0.0	2.50
1-C	Glass + 0.1 clay	0.5	0.5	0.1	2.70
1-D	Glass + 0.2 clay	0.5	0.5	0.2	2.90
1-E	Glass + 0.3 clay	0.5	0.5	0.3	3.10
1-F	Glass + 0.4 clay	0.5	0.5	0.4	3.30
1-G	Glass + 0.5 clay	0.5	0.5	0.5	3.50

These mixtures were each prepared, mixed with dextrine, molded into cones, dried, and tested against cones 010 to 1 as before. To our great surprise, No. 1-B, consisting of pure glass alone, stood up till cone 03 was down flat, and cone 02 was bent to a semi-circle, point touching. At the same time, mixtures 1-C and1-D were almost as much affected as 1-B, while 1-E was slightly bent and F and G were not yet affected. This showed that the melting point of common glass, if fairly represented by our sample, was altogether too high for our purpose, and also that a cone in which glass was the principal part, constituting 70 per cent. or more of the mixture, melted substantially as if it were pure glass. It requires more than 30 per cent. sterile or infusible matter to sensibly reduce the melting of the glass mixture.

This second test established the fact that no help was to be expected from common glass as a flux, at temperatures much below where feldspar becomes available, which completely removes it from the list of available supplies for our purpose. This fact is directly contrary to our previous comparisons, and indeed the idea seems general among ceramists that common glass is a valuable aid as a flux, where feldspar and Cornwall stone are too hard, and where lead is not desired. The writer is personally familiar with several cases where attempts have been made to use it in stoneware glazes, or as a body flux, but in every case with similarly disappointing results.

A comparison of the formulae of the two substances shows us that there is no chemical reason for expecting to derive any great advantage from the glass, viz.:

Feldspar ..... $K_2O, Al_2O_3, 6\ SiO_2$.
Common glass.. $Na_2O, CaO, 5\ SiO_2$.

and the results are therefore reasonable, though contrary to the general impression.

IV. Influence of Changes in Total Acidity, Maintaining

$$Na_2O : CaO :: 1 : 1.$$

The query now arose as to whether common glass represents the most fusible silicate which can be made from the ingredients of which it is composed, viz., Na_2O, CaO and SiO_2. Seger has stated that the glass-maker must remain between the ratios RO, 2 SiO_2 and RO, 3 SiO_2, because the glass becomes too fluid below the former, and too pasty above the latter extreme. To test this matter, the following series was devised:

TABLE II.

No. of Glass.	Molecular Composition.			Oxygen Ratio Base : Acid.
	Na_2O	CaO	SiO_2	
1-H	0.5	0.5	2.50	1 : 5
1-I	0.5	0.5	2.00	1 : 4
1-J	0.5	0.5	1.50	1 : 3
1-K	0.5	0.5	1.00	1 : 2
1-L	0.5	0.5	0.75	1 : 1.5
1-M	0.5	0.5	0.50	1 : 1

These glasses were prepared by mixing two batches, 1-H and 1-M. 1-H consisted of 53 parts of common soda ash (Na_2CO_3), 50 parts of Paris white, 150 parts of potter's flint, all of which were ground intimately together in a dry ball mill. No. 1-M consisted of the same quantities of soda and lime, with only 30 parts of flint. From these two extremes, the other four glass batches were prepared by blending:

No. 1-I=3 parts, 1-H and 1 part 1-M.
No. 1-J=2 parts, 1-H and 2 parts 1-M.
No. 1-K=1 part, 1-H and 3 parts 1-M.
No. 1-L=1 part, 1-H and 7 parts 1-M.

Each batch was weighed, mixed thoroughly by hand, put two or three times through a sieve, to prevent lumps of any ingredient forming, and finally put into a large white-granite coffee cup, which served as crucible for the melt. The cups were all placed in an up-draft kiln, and fired to cone 8. No covers were used, so that the behavior of the glasses during fusion could be observed. None of them swelled up much, or foamed in fusion; not one overflowed its cup.

After cooling the kiln, the cups were removed, broken, the glass cracked out in large or small pieces, and carefully sorted to remove all particles of the walls of the cup, which shivered in cooling and adhered to the glass. This was a tedious task, and took about 1½ or 2 hours per sample The clean glass was then crushed to sand, and pulverised in a mortar, until it passed through a 100 mesh screen, dry, when it was put into bottles and labeled. The process could have been somewhat hastened by removing the cups from the kiln hot, and pouring the fluid glass into water, and thus crackling it, but it was though better to cool slowly, in order to procure and preserve a sample of each glass or slag for future reference.

The silicates produced in this series were by no means uniform in appearance.

No. 1-H—Common window glass formula.

This was a fairly-well fused, light colored glass, full of a multitude of very minute bubbles which the glass was too

viscous to disengage. Evidently, cone 8 is too low a temperature to permit this glass to become very fluid or thin.

No. 1-I — Representing Seger's glass of minimum acidity.

This was a clear brilliant glass, almost perfectly free from bubbles of enclosed gases. Color same as before. No crystals or segregations were found.

No. 1-J—This and subsequent silicates are below the ratio prescribed by Seger.

This silicate was still a clear, transparent, light colored glass, free from bubbles of enclosed gases. It showed on its surface a number of large white crystals of the Isometric system, some of them as much as ⅛ of an inch across, and extending down into the glass $\frac{1}{16}$ from the upper surface. The matrix surrounding the crystals was perfect glass.

No. 1-K—This silicate was an almost opaque, greenish white slag, of vitreous surface and fracture. It had changed very much indeed from the preceeding, and looked as if it were wholly composed of the same silicate which composed the white crystals found on the surface of 1-J.

No. 1-L—This was a slate gray, opaque, vitreous silicate; hard, but could be cut with a good knife. It gave evidence of crysralline structure when examined closely, but appeared otherwise like a stone. There were more or less bubbles in it, but the mass was not strongly vesicular. Its appearance suggested that it had been well fused, but not that it had been notably fluid.

No. 1-M—This is a rough, coarsely vesicular, silicate, composed of various colors—gray and bright sky-blue predominating. It appears wholly stony at first sight, but on close examination, is seen to be a mass of crystalline matter, some of the crystals being clearly distinguished by the unaided eye. It was not apparently well fused, and was the only one of the series giving evidence of too little fusion. The colors are distributed irregularly, and the blue is a prominent factor in it. What this mineral is, cannot be even guessed, but it seems evident that the amount of silica present is too small to actively engage all of the Na_2O and the CaO, and hence several compounds, of quite diverse

character, have been formed and are mingled in the slag.

Nothing conclusive could be determined from the appearance of the melts as to their relative fusibility, so the following test was undertaken:

From these glasses, after powdering as described before, cones were made by mixing the glass dust with dextrine paste and molding, as usual, in steel molds. The mixture had neither plasticity nor strength, until dry, but when once dried out the cones were tough and could be handled with ease and safety. Two sets of these were placed on slabs of clay, as usual in cone tests, and put into a double-deck updraft muffle kiln and fired. The kiln was provided with a Chatelier thermocouple, and galvanometer, and temperatures were carefully noted at all critical points. Cones 03, 05, 07 and 09 were also placed in the test. The contents of the lower muffle, being nearer the fire, were first to fuse, and when these cones were down, those in the upper muffle began to go, so that two sets of reading were obtained.

In the lower muffle No. 1-H and No. 1-I were both carried down, and No. 1-J partly down, by a flame striking them for a few minutes, after freshly baiting the fire. This occured early in the burn, and while the general temperature of the kiln was very low, the pyrometer registering only 560 deg. C. The temperature of the flame which carried the cone down was not indiciated by the pyrometer, and is a matter of speculation only. After stopping the flame, and bringing the heat up uniformly, no further change in the glass-cones was produced until Cremer cones 09 and 07 were down and 05 started. At this point, J got completely down, and K followed almost at once.

In the upper muffle, cone 1-I began to go first, followed by J and K. All of these went down partly, before Cremer cone 09 started, but the latter and 07 got down flat, while the glasses were still only bent, Nos. L and M. were unaffected in both muffles,

The test, though not wholly satisfactory, showed that glass 1-I was probably the softest, being distinctly ahead of No. 1-H and somewhat ahead of No. 1-J. The series showed clearly that there was no hope of getting any softer glass

by depressing the acidity below the oxygen ratio 1 : 3 ; in fact, a very marked increase in refractoriness began at once, and the melts showed this by loss of the glassy, and development of the stony, structure.

At the same time, when series III was projected, it was our expectation, based on the statements of Seger, that a marked reduction of the total acidity of the glass would produce exceedingly fusible glasses or slags. It was thought that with such a soda lime-silica compound, it would be desirable to use some Al_2O_3 in the fusion, if possible, both to prevent devitrification and to make a formula in which the Al_2O_3 could be increased from a minimum up to 0.5 equivalents in cone 4:

Without waiting to see the results of series III, and thus proceed in logical order, we attempted to jump at conclusions, with the usual results.

Series IV was formulated as follows:

TABLE III.

No. of Glass.	Molecular Composion.				Oxygen Ratio Base : Acid.	Molecular Ratio $Al_2O_3 : SiO_2$
	Na_2O	CaO	Al_2O_3	SiO_2		
1-N	0.5	0.5	0.00	0.50	1 : 1	1 : ∞
1-O	0.5	0.5	0.10	0.65	1 : 1	1 : 6.5
1-P	0.5	0.5	0.20	0.80	1 : 1	1 : 4.0
1-Q	0.5	0.5	0.30	0.95	1 : 1	1 : 3.1
1-R	0.5	0.5	0.40	1.10	1 : 1	1 : 2.75
1-S	0.5	0.5	0.50	1.25	1 : 1	1 : 2.50

These mixtures were produced as in the preceding case, by blending between extremes.

No. 1-N—Was composed of 53 parts soda ash, 50 parts whiting and 30 parts of flint. No. 1-S was composed of the same quantities of soda and lime, 129.5 kaolin and 15 of flint. The intermediate blends were made in the proportion 4 : 1, 3 : 2, 2 : 3, and 1 : 4 respectively.

On melting at cone 8 as before, and cooling slowly, the melts presented the following appearance:

1-N—Same as 1 M of series III.

1-O—A mark grayish slag, with small sky-blue patches, same color as those in M, distributed irregularly through it;

texture is stony, and somewhat vesicular. No hint of gla s. iness about the specimen.

1-P—A magnificent emerald-green glass, free from inclosed gas bubbles. Surface is a scummy, and a few little segregations of pearly white matter lying on the bottom of the melt.

1-Q—A mixture of green glass like O, with large amounts of pearly white, opal-like segregations, The mass of the two materials is about equal. The opal-like substance may be simply undissolved kaolin, or alumina which has given its silica up to the stronger bases, soda and lime, or it may be some other material, segregated from a homogeneous magma in cooling. The individual particles of it were half as large as peas, surrounded with the matrix of green glass.

1-R—Stony, vesicular slag with no trace of glassiness.

1-S—Ditto.

This series was like-wise tested in cone form in the kiln, in order to see whether any or all of its members were fusi: ble at low temperatures. The test showed that up to temperatures of cone 02, not one of the slag-cones had showed any signs of fusion, while other glasses, more silicious and less aluminous were melting freely around them.

This series does not prove anything directly of service to this investigation, so far as the influence of alumina on the fusiblity is concerned, because neither 1-M nor 1-N were melted, and hence the aluminous and non-aluminous glasses of oxygen ratio 1 : 1 stands in the same position. It does show, however, that the addition of Al_2O_3 does not *increase* the fusibility of the melts materially, at least in compounds already so basic.

Nevertheless, a well known principle is beautifully illustrated in the consequence; *i. e.*, the power of alumina to prevent devitrification.

Melt N, markedly crystalline even to the naked eye, and stony in fracture, becomes crypto-crystalline in O, a perfect glass in P, a glass full of suspended insoluble matter in Q, and a stony mass again in R and S. The amount of Al_2O_3 in the last two numbers of the series was evidently as much too great, as it was too little in N and O.

On the completion of series III, it became evident that it was necessary to learn more in detail the absolute point of greatest fusibility between glasses 1-H and 1-J. Also it seemed desirable to check the results of series III by a confirmatory series. Accordingly series V was designed as follows:

TABLE IV.

No. of Glass	Molecular Composition			Oxygen Ratio Base : Acid
	Na_2O	CaO	SiO_2	
1-T	0.5	0.5	2.50	1 : 5.0
1-U	0.5	0.5	2.25	1 : 4.5
1-V	0.5	0.5	2.00	1 : 4.0
1-W	0.5	0.5	1.75	1 : 3.5
1-X	0.5	0.5	1.50	1 : 3.0

These were made up as before, and blended and melted by a different person, so that no personal errors would be likely to be duplicated. The temperature went to cone 8 as before.

The glasses were in all respects like those of identical constitution in series III. No. 1-X which was a duplicate of No. 1-J, even had the same white isometric crystals on its surface, showing that this was no freak, but a strong and consistent tendency in a silicate mass of this constitution, to split up into definite molecular compounds, one of which happened to be opaque, and thus visible.

On pulverizing and fashioning into cones, and testing in the kiln, the following interesting results were obtained:

TABLE V.

No. of Glass	Composition	Time of Fusion	Temp. Deg. C	Remarks
1-V	RO, 2.00 SiO_2	5:50 P. M.	925	Undoutedly flashed
2-C	RO, 2.00 SiO_2	8:25 P. M.	1075	Fusion normal
1-U	RO, 2.25 SiO_2	8:55 P. M.	1125	.....
1-W	RO, 1.75 SiO_2	.....	.....	Tipped over before fusion took place
1-X	RO, 1.50 SiO_2	8:55 P. M.	1125	
1-T	RO, 2.50 SiO_2	9:05 P. M.	1150	

These results clearly proved that the RO, 2 SiO_2 glass

is distinctly softer than those above it, or well below it, but owing to the incidental loss of 1-W, did not show that the desired minimum did not lie somewhere between RO, 2 SiO_2 and RO, 1.75 SiO_2.

Accordingly the series was again made up and fired, with the following results:

TABLE VI.

No. of Glass.	Composition.	Time of Fusion.	Temp. of Fusion.
1-V	RO, 2.00 SiO_2	6.11 P. M.	1169 deg.
1-W	RO, 1.75 SiO_2	6.11 P. M.	1169 "
1-X	RO, 1.50 SiO_2	6.16 P. M.	1170 "
1 U	RO, 2.25 SiO_2	6.19 P. M.	1192 "
1-T	RO, 2.50 SiO_2	6.25 P. M.	1210 "

The temperatures recorded were much higher in this burn than in the last, but in neither case, were they corrected for atmospheric temperatures outside the kiln, and hence must not be considered as having a close comparative value. They were only used to indicate the relative temperature intervals between fusions, and not the absolute melting points. The scanty time and temperature intervals in this test, probably resulted from injudicious firing at this point, as the rise in temperature was very rapid.

These two burns showed that RO, 2 SiO_2 is at least as soft as any glass of same RO, either more or less acid, and with some presumptive evidence in favor of its being the point of actual minimum.

This fact alone is of great interest and value, and is directly at variance with the ideas which have been generally held on this point. In metallurgy for instance, we have been taught for many years past that most slags find their most fusible ratios between the meta- and ortho-silicates, or between oxygen ratios of 1 : 2 and 1 : 1. And in ceramics, Seger has announced that RO, 2 SiO_2 is the lower extreme of glass composition, because ratios lower than that produce glasses too liquid and too little viscous to work well in glass blowing.

But here we have a glass, whose melting point is lower at an oxygen ratio of 1 : 4, than at 1 : 3 or 1 : 2 or 1 : 1

Seger's statement must have been based on the fact that a softer glass was obtained by each successive diminution of acid down to this point, and without pushing his tests further to see that he was then at the point of maximum fusibility.

Variations in Lime-Soda Ratio.

Having established the degree of acidity at which this RO combination (0.5 Na_2O 0.5 CaO), is most fusible, and having found it so little suitable to the needs of the situation, the next line of attack was through the variation of the RO elements. It was known in a general way that the silicates of soda alone were much more fusible than those of lime. The point to determine, then, was, was whether increase of Na_2O and decrease of CaO in this RO, 2 SiO_2 ,would materially depress its melting point. To develop this information the following series No. VI was made:

TABLE VII.

No. of Glass.	Molecular Composition.			Appearance after fusion at cone 10.
	Na_2O	CaO	SiO_2	
1-Y	1.00	0.00	2.00	Clear, greenish-yellow, can be cut with knife.
1-Z	0.85	0.15	2.00	Clear, bluish color-harder than Y
2-A	0.70	0 30	2.00	Clear, bluish glass, full hardness.
2-B	0.60	0.40	2.00	Clear, bluish glass.
2-C	0.50	0.50	2.00	Lighter color, more gas bubbles in it.
2-D	0.25	0.75	2.00	Semi-translucent, bluish white glass.
2-E	0.00	1.00	2.00	Porous, white solid; vitrified only on exterior parts, where heat was highest.

There was a very marked difference in time of fusion of the members of this series. The high-soda glasses melted very early and quietly, while the high lime end either did not melt at all, or melted slowly. Obviously, no help can come from 2-C, 2-D, or 2-E. In view of the fact that the increase of the alkali in the glass is at the expense of its stability before the attacks of water and other solvents, it seemed clear that a limit in this case would be found from

this cause, if no other. It was argued that if a glass cannot resist the attacks of water, how can we expect it to resist the solvent and distructive action of acid dew in water-smoking kilns, or of SO_3, at temperatures between 300 and 900 deg. C.

To determine this matter of solubility with sufficient exactitude for the scope of this enquiry, one gram of each glass, previously powdered and put through a 100 mesh screen, was treated with 50 c.cs of distilled water, on a water bath for 10 hours with frequent stirring. Finally the liquids were filtered, and the filters washed with hot water in equal amount. The clear solutions were then evaporated to dryness in tared vessels, and the weights of solid residue determined.

The figures are as follows:

TABLE VIII.

No. of Glass.	Molecular Formula.	Per cent dissolved in hot water.
1-Y	1.00 Na_2O, 0.00 CaO 2 SiO_2	100.00
1-Z	0.85 Na_2O, 0.15 CaO 2 SiO_2	24.56
2-A	0.70 Na_2O, 0.30 CaO 2 SiO_2	9.88
2-B	0.60 Na_2O, 0.40 CaO 2 SiO_2	4.43
2-C	0.50 Na_2O, 0.50 CaO 2 SiO_2	4.21
2-D	0.75 Na_2O, 0.25 CaO 2 SiO_2	4.15
2-E	0.00 Na_2O, 1.00 CaO 2 SiO_2	not determined.

These figures clearly show that No. 2-B represents the maximum soda content which such a glass may have, without material sacrifice of its permanence and stability under the attack of solvents. This point has no definite connection with the behavior of the glasses at high temperatures and in acid gases, which is the conditions they must meet in kiln firing, but it constitutes a basis for distrust of the high-soda glasses, at least.

To ascertain their bearing under kiln conditions the ground glasses were made up into cones as before, and fired with the following results:

TABLE IX.

No. of Glass.	Lower Muffle.		Upper Muffle.	Remarks.
	Time of Fusion.—P. M.	Temp. of Fusion-Deg.	Time of Fusion.—P. M.	
1-Y	5.15	855	Betw'n 6 & 7	
1-Z	Fell over before fusing	....	7:55	
2-A	5.50	925	8:25	Cone 010 melted
2-B	7.45	1040	9:05	at 9.30 P. M. in
2-C	8.25	1075	9:20	upper muffle.
2-D	9.35	1250	did not fuse.	

The soda glass, 1-Y, showed its marked fusibility over any of the others in both upper and lower muffles, but the readings gotten from it were very difficult to interpret, as it did not fuse regularly, but simply softened and swelled until finally it was a spongy fluid. The time periods recorded represent the best judgement that could be formed under the circumstances. In as much as it is one of the faults of the Cremer frit, that it swells and appears to give off gases in fusion, and that it is to avoid this fault, in part, that these experiments have been made, it is evident that glass 1-Y and probably 1-Z must be ruled out for this reason if no other. But the solubility table shows both should be ruled out on those grounds also, and that 2-A is the highest soda glass worth further study.

To get a check on the results just given, as well as to establish the connection between the melting points of series V and VI, another burn was made, in which two cones each of No. 2-A, B, C and No. 1-T-U-V-W-X were placed in one slab. The results are appended:

TABLE X.

No. of Glass	Molecular Composition			Aver'ge Time of Fusion P. M.	Aver'ge Tem. of Fusion deg C
2-A	0.70 Na_2O	0.30 CaO	2.00 SiO_2	4:36	980
2-B	0.60 Na_2O	0.40 CaO	2.00 SiO_2	5:30	1083
2-C	0.50 Na_2O	0.50 CaO	2.00 SiO_2	6:13	1169
1-V	0.50 Na_2O	0.50 CaO	2.00 SiO_2	6:11	1169
1-W	0.50 Na_2O	0.50 CaO	1.75 SiO_2	6:11	1169
1-X	0.50 Na_2O	0.50 CaO	1.50 SiO_2	6:16	1170
1-U	0.50 Na_2O	0.50 CaO	2.25 SiO_2	6:19	1192
1-T	0.50 Na_2O	0.50 CaO	2.50 SiO_2	6:25	1210

Cone 010 was also placed on this slab, and fused at 5.13 P. M., at 1055 deg. C. or about 100 degrees above its schedule temperature. This reading is not corrected, and hence has a relative value only; but it shows by increase of alkali and decrease of lime, it is quite possible to make a soda-lime glass more fusible than cone 010.

The net results of this enquiry now stand:

1st. Using one-half molecule each soda and lime, 2.00 molecules of SiO_2 makes the most fusible glass producible.

2nd Diminishing the lime, and increasing the soda to

$$\left.\begin{matrix}0.7\ Na_2O\\0.3\ CaO\end{matrix}\right\}2.00\ SiO_2$$

the melting point falls from 1169 degrees to 980 in one test and from 1075 to 925 degrees in another, or a net gain of over 150 degrees C.

But owing to doubts about the stability of a glass so high in alkali, it was deemed wiser to use 2-B.

$$\left.\begin{matrix}0.60\ Na_2O\\0.40\ CaO\end{matrix}\right\}2.00\ SiO_2$$

as the basis for further tests.

This glass had melted in the last burn, practically with cone 010—inthe lower muffle a little before, and in the upper muffle, a little after. Now, to use such a glass as the basis of cone 010 would be impossible, as large dilution of the basal frit is necessary in cone making in order to bring about gentle and uniform fusion. At the very least, a glass should melt at 200 deg. C below cone 010, in order to permit of sufficient dilution to make that cone work well.

Hence, it will be seen that glass No. 2-B was far from filling our needs in point of fusibility. At the same time, it must be noted that we had brought the melting point down from cone 02-03 to cone 010-09, by the tests so far prosecuted.

VI The most favorable ratio of Alkalies K_2O : Na_2O.

It was now evident that no compound of soda, lime and silica could be produced of the necessary fusibility, without sacrificing other qualities. The question then arose as to the effect of substitutions from the list of availables given before.

It was first determined to try the effect of substitution

of potash for soda and then, the use of the two alkalies jointly. Accordingly the following series VII glasses were made up.

TABLE XI.

No. of Glass.	Molecular Composition.		
2-T	0.6 K_2O	0.4 CaO	2.00 SiO_2
2-U	0.6 Na_2O	0.4 CaO	2.00 SiO_2
2-F	0.3 K_2O 0.3 Na_2O	0.4 CaO	2.00 SiO_2

These were made up from potassium carbonate (c. p.), soda ash (commercial) whiting, flint; were fused in cups to cone 8, and were then broken, sorted, crushed, put through 100 mesh sieve, and made into cones as before. These were tested along with others, but for the sake of clearness the results are treated separately:

TABLE XII.

No. of Glass.	Composition.	Fusion Data.		
		Lower Muffle.		Upper Muffle.
		Time. P. M.	Temp. Deg C.	Time—P. M.
2-U	Potash-Lime glass	7:00	945	7:22
2-U	Soda-Lime glass	6:50	925	7:26
1-F	Potash-Soda-Lime glass	6:36	890	7:10

Cone 010 fused at 6:56 p. m. in the upper muffle.

It will be noted that longer time, or heat soaking, has caused the potash and soda mixtures to change places in the order of fusibility, while the potash-soda cone melts distinctly lower in each case, than that of either alkali alone.

The point seems clear that the mixture of two alkalies is better than either alone. This point is well known in other connections, viz., in fluxing in platinum crucible in the chemical laboratory, and in making slags in furnaces and in substituting other bases for lead oxide in glazes, etc., etc. But so many contradictions had been met so far since undertaking this work, that it was thought best to take nothing for granted, and to prove each point as we came to it.

VII. Resort to Other Bases Than Lime.

The reduction in fusing temperature from all preceding causes was too little, however, to materially affect the problem. Hence, a new series, No. VIII, was perfected as follows:

TABLE XIII.

No of Glass.	Molecular Composition.				Mixture.
	K_2O	Na_2O	RO	SiO_2	
2-F	0.3	0.3	0.4 CaO	2.00	The K_2CO_3, Na_2CO_3 and SiO_2 were ground together in dry ball mill, and then to equiv. parts of this mixture, the respective oxides, or carbonates were added.
2-G	0.3	0.3	0.4 MgO	2.00	
2-H	0.3	0.3	0.4 BaO	2.00	
2-I	0.3	0.3	0.4 ZnO	2.00	

The glasses resulting from this fusion were all of excellent quality. The calcium glass was the least brilliant of the lot, and the zinc and barium glasses were of especially high refractive index and therefore brilliant to the eye. In fusion, the barium glass fused first, and the calcium glass last, but this could not be accepted as evidence of the fusibility of the glasses when once formed, though of course it gives us a basis for expectation.

On putting these through the usual procedure of grinding, fusion, sorting, regrinding, cone molding and firing, the following figures were gotten:

TABLE XIV.

No of Glass.	Composition.	Fusion Data.		
		Lower Muffle.		Upper Muffle.
		Time. P. M.	Temp. Deg. C.	Time—P. M.
2-F	Lime-Glass	6:36	890	7:10
2-G	Magnesia-Glass.	4:42	755	6:30
2-H	Barium-Glass	6:29	880	7:16
2-I	Zinc-Glass	4:42	755	6:12

These figures alone fail to accurately represent the facts, because the fusion of a glass is often a slow process, and a cone may begin to soften and bend a long time before it

actually fuses, and uncertainly arises as to which may be fairly considered having fused.

For instance, in the above case, the Barium glass 2-H, softened at 2:55 p. m., and bent to a perfect semi-circle, apex touching the level of the base. This was done during a sudden flash of flame, and while the average temperature of the muffle was below 600 deg. C. However, the other cones had the same exposure as the Barium glass and did not show any signs of fusion, while both of the Barium cones, one in the front row nearest the flame, and the other in the rear row, farthest from the flame, went down as described with only one minute's interval. Having gone down in this decided manner, and clearly given notice of some sort of molecular change, they stood in the arched form without further softening, until 6:29 p. m. when they collapsed. During this time the magnesium glass softened at 3:19 p. m., and fused at 4:42 p. m.; and the zinc glass softened at 4:15 p. m., and fused at 4:42 p. m. The calcium glass did no begin to fuse till 6:34 p. m., and was down flat at 6:36 p. m.

This evidence is undoubtedly somewhat confusing. The only thing which is beyond doubt is that the glass

$$\left.\begin{array}{l} 0.3\ K_2O \\ 0.3\ Na_2O \\ 0.4\ CaO \end{array}\right\} 2\ SiO_2$$

is the least fusible of any of the four under test. The Barium may be considered as being the softest or next to the hardest, according as the first softening or final fusion is considered the more important,

In the upper muffle, the evidence was less doubtful. The zinc cone was here distinctly first down, followed by the magnesia cone, then Barium and lime respectively.

The situation was now improved to the extent that three bases had been tested, and all made glasses notably softer than lime glasses of the same proportions. Of these, the zinc and magnesia glass were beyond all question much softer than cone 010, and the Barium glass either much softer or about as soft as cone 010.

Without waiting to carry the further comparison of these glasses to its final conclusion, it was thought best to push on

and see if any further notable reductions in temperature of fusion could be gotten by making still more complex glasses, using two or more of the bases together. It was thought that even after sufficiently fusible glasses were found, that there were other problems to be considered in adopting one for final use as the base of a cone series, and therefore it would pay to run over the ground tentatively and find what general combinations gave the greatest promise of developing the required combination of properties.

Accordingly series IX was formulated as follows:

TABLE XV.

No. of Glass	Molecular Composition.						
	K_2O	Na_2O	CaO	MgO	BaO	ZnO	SiO_2
2-J	0.8	0.8	0.2	0.2	....	....	2.00
2-K	0.8	0.8	0.2	....	0.2	....	2.00
2-L	0.8	0.8	0.2	....	....	0.2	2.00
2-M	0.8	0.8	....	0.2	0.2	....	2.00
2-N	0.8	0.8	....	0.2	....	0.2	2.00
2-O	0.8	0.8	....	....	0.2	0.2	2.00

In this series, the K_2O, Na_2O and SiO_2 were weighed out and ground intimately together as one batch, and then divided into small portions, into the which proper quantities of the two remaining oxides were then added and ground in.

The melts resulting were all clear brilliant, handsome glasses, any of which seemed perfectly suited to the usual uses. Those in which barium and zinc were used were brighter than the others, perhaps. The first attempt to make this series was a failure, on account of two defects. First, the glasses foamed and boiled over in fusion, so that but little of some of the melts remained. Second, the wind being high, considerable quantity of iron rust was disengaged from the stack, and got down into the kiln, and into the glass crucibles. Accordingly, the entire series was prepared again, and melted, this time successfully.

It was noted in watching the glasses fusing, that 2-M was considerably in advance of any other in fusing. It was the first to begin to raise from the expulsion of gases, and the first to settle back and begin to clarify. This seemed

significant to us, and led us to expect an equally marked fusibility when the glass was tested in cone form.

The glasses were then pulverized, and made into cones as before, and tested with the results set forth in the succeeding table:

TABLE XVI.

No. of Glass	Variable Elements	Lower Muffle		Upper Muffle	Remarks
		Time of Fusion p. m.	Temp. of Fusion deg.	Time of Fusion p. m.	
2-J	CaO, MgO	5:35	755	*	Undoubtedly less fusible than the other glasses
2-K	CaO, BaO	5:55	800	*	
2-L	CaO, ZnO	5:45	780	6:38	Began latest to fuse
2-M	MgO, BaO	5:20	705	6:39	Bloated in both cases
2-N	MgO, ZnO	5:35	755	6:38	Fused well, did not swell
2-O	BaO, ZnO	4:45	575	6:28	Fused well and regularly

Cone 010, Cremer Series, fused at 7:22 p. m., at 945°C.

Only one feature is salient in the preceding table and that is: None of the glasses containing calcium fuse as readily as those in which it is absent. This agrees with the finding of Series VIII, in which calcium formed the least fusible glass. In this last series, the calcium seems to handicap the mixture in which it occurs, very markedly, even when linked with barium which is supposed to have such strong fluxing powers.

The rest of the series are difficult to fairly evaluate. The barium-zinc glass seemed to fuse first in both muffles; but it was not earlier to start than the barium-magnesia glass. It finished earlier, because the latter swelled and bloated before fusion was complete, and thus obscured its end-reaction.

The series also seemed to show that a distinct reduction in fusion point could be produced by using 0.2 equivalents of two fluxes, rather than 0.4 of any one. This is in accord with the general prevailing idea, and with what was specifically proven in regard to K and Na.

Another significant feature of this burn was the fact that the cones in the upper muffle had far less time interval than

* * 2-J and 2-K did not fuse in upper muffle.

the same cones below. The lower cones were fused after a shorter, sharper fire; the upper cones had a longer exposure to sulphuric kiln gases, and this seemed to have had an effect in diminishing the natural difference between them.

To still further probe the matter of the effect of mixture of RO elements, another series, No. X, was prepared as follows:

TABLE XVII.

No. of Glass	Molecular Composition						
	Na_2O	K_2O	CaO	MgO	BaO	ZnO	SiO_2
2-P	0.3	0.3	0.13	0.13	0.13	....	2.00
2-Q	0.3	0.3	0.13	0.13	....	0.13	2.00
2-R	0.3	0.3	....	0.13	0.13	0.13	2.00
2-S	0.3	0.3	0.10	0.10	0.10	0.10	2.00

On fusion, these produced handsome brilliant glasses of high refractive index. Especially noteworthy was 2-R, which had a delicate bluish cast and a beautiful, limpid, yet brilliant appearance. On making into cones and testing, they showed as follows:

TABLE XVIII.

No. of Glass	Fusion Began		Fusion Completed		Remarks
	Time p. m.	Temp. deg. C.	Time p. m.	Temp. deg.	
2-P	4:14	725	4:21	735	A very satisfactory test. No swelling or failure to bend properly.
2-Q	4:14	725	4:37	795	
2-R	4:14	725	4:30	775	
2-S	4:08	700	4:26	755	
Cone 010	5:10	880	5:16	910	

These results seem perfectly decided, and unimpeachable, whatever we may think of their rationality.

In order, now, to correlate the last three series, it was decided to select the three softest glasses from each series and melt them all at once on one slab. This was done with the following results:

TABLE XIX.

No of Glass	Variable Element	Fusion Data				Remarks
		Fus'n Began		Fus'n Ended.		
		Time p. m.	Temp. deg.	Time p. m.	Temp. deg.	
2-G	MgO	5:22	670	5:50	720	Swelled.
2-I	ZnO	5:22	670	6:00	755	Did not swell.
2-M	MgO, BaO	5:14	650	5:40	700	Swelled badly.
2-N	MgO, ZnO	5:40	700	....	....	Swelled.
2-O	BaO, ZnO	5:14	650	5:22	....	Swelled.
2-P	CaO, MgO, BaO	5:14	655	5:40	700	
2-Q	CaO, MgO, ZnO	5:43	705	6:00	755	Swelled little.
2-R	MgO, BaO, ZnO	5:43	705	5:50	720	Swelled.
2-S	CaO, MgO, BaO ZnO	5:43	705	5:50	720	Swelled.
No. 010	Cremer Cone.	6:25	865	6:39	925	

This series of results was exceedingly unsatisfactory and was probably due, at least in part, to an experiment. It had been noticed in all the cones composing Series VIII, IX and X, that is, those in which MgO, BaO, and ZnO partly or wholly replaced CaO, that a tendency was evinced to swell up in fusing, and instead of proceeding at once to clean-cut fusion, to form a puffy mass in which no definite end reaction could be gotten.

VIII, Efforts to Locate Cause of Bloating.

This swelling must of course be ascribed to the liberation of some gas in the process of fusion. What gas it can be is a question more easily asked than answered. The first thought was sulphur, that "bete noir" of all ceramic operations. But, as the glasses were perfectly clear after fusion, and showed no sign of crystalline segregations, such as sulphates often cause, it was thought that they must have absorbed it during the cone burn, rather than while the glass itself was being fused in the beginning. This idea was strongly supported by the fact that where cones were fired in both the lower and upper muffles, the latter always showed less time interval between the glasses than below. This would be the case if they had been sulphured a little, as the infusible crust forming on the out side, would cause the cone to stand with but little progress until finally the shell wonld

fuse and let the fluid contents very rapidly assume a level state. This reaction was repeatedly witnessed, and yet was markedly absent in some of the cones.

Acting on the hypothesis of absorption of SO_3 from the kiln gases, it was thought that if the cones could be introduced into the kiln after the latter had reached 550°C, or visible red heat, that the long delay in getting started would be avoided, and the glasses would fuse before they had an opportunity to absorb much sulphur. Accordingly the kiln was heated up to redness, and the cone slab was introduced, well dried and heated to 150°C on a hot iron plate. This was neatly done and the pyrometer showed the presence of the comparatively cold slab near it, as follows:

	Temp. deg.	Time p. m.
Before slab was introduced.....	615	4:15
Slab put in.....	560	4:17
	590	4:22
	600	4:27
Slab caught up with kiln here.....	615	4:31

The results of this maneuver were anything but conconvincing however, as the cones bloated in greater number and in greater amount than any preceding test. It seems evident that the SO_3 or whatever gas it may be which is causing this difficulty, is a part of the glass, and indeed that slow fusion or slow pre-heating rather favors than hinders its escape.

The results attained were very meager and uncertain on account of the bloating. It seemed tolerably evident that 2-M, 2-O and 2-P, were the three most fusible glasses in the test, though 2-G and 2-I are close behind them. 2-R, 2-S and 2-Q were distinctly less fusible, 2-Q probably the least fusible of any tested on the slab.

In order to test the reverse treatment as to absorption of SO_3, another set of the same cones was mounted on a slab, in the same relative positions, and fired. This time the cones stood in the open kiln 5½ hours before they began to soften. The kiln gases were maintained oxydizing throughout. The results were very much better in every way. The glasses did not swell, and bent gently over till they formed arches, which then collapsed into a puddle, after a little delay.

The results of this burn were as follows:

2-M, 2-O and 2-P were again the softest glasses, closely followed by 2-G and 2-I. 2-R and 2-S showed better in this test, though still behind those named before, and 2-Q was again definitely the hardest glass in the lot. The results exactly check those of the preceding test, though much more satisfactory in execution.

IX. Summary of Results to this Point

The work accomplished up to this point may be summarized as follows:

It has been shown, (A) by reduction of SiO_2 from 2.50 to 2.00 molecules, (B) by increase of alkali from 0.5 to 0.6 molecules, (C) by use of 0.3 K_2O and 0.3 Na_2O, in place of 0.6 of either alone, (D) by replacement of CaO, either wholly or very largely, with MgO, BaO or ZnO or mixtures of these, that a large variety of glasses have been produced which soften at about 650 degrees, and melt at 750 degrees, while Cremer cones 010 placed in the same test, melt from 875 to 925 degrees. Common window glass required in another burn, cone 02, before fusion, in which the pyrometer showed the abnormally high temperature of 1155 degrees. It is not likely that the figures are strictly comparable, but at least, there is no doubt that the glasses produced at the close, melted fully 350 degrees, and possibly 400 degrees below the common window glass first tested.

This statement shows in a word, that real, solid, progress has been made. We have now, not one, but many glasses, any of which fuse 150 to 200 degrees below cone 010, and so far as mere fusibility is concerned, could be used as the basis of a cone 010 mixture. Also, the progress has been attained without recourse to any but non-reducible oxides, as originally planned.

By no means all the work has been yet accomplished, however.

(E) The most fusible members of these preceding glasses has not yet been isolated, and the behavior of most of them has been contradictory and misleading in some one of these tests. It seems assured that Barium will be a constit-

uent of the softest glass, but beyond this we cannot now say.

(F) We do not know the cause of the swelling of these glasses when fired as cones. Whatever the cause, if it cannot be overcome, it will go a long ways towards spoiling the glasses for use as the basis of a cone series. The Cremer frit has this fault, and it is in part to escape this that this work was begun.

(G) We do not know whether these glasses, any of them, are less volatile under long continued heat than the Cremer frit. Their composition gives us the right to expect them to be, but proof is still lacking.

It seemed unadvisable to proceed with further tests under E, to determine the glass of the greatest absolute fusibility, until we had answers to F and G, for we had no assurance that the most fusible glass might not be the one with the greatest affinity for absorbing gases, and for volatizing its own constituents. It is quite within the possibilities of the case, that a glass may ultimately be selected, which is considerably less fusible than the minimum, but whose stability makes it safer as a base-frit for the cone series.

X. The Ultimate Cause of Swelling During Fusion.

Taking up the consideration of topic F, the attempt was next made to determine the cause of the gaseous evolution, by which the more fusible cones have many of them swelled during fusion. The hypothesis has already been advanced, that sulphur in some form, probably that of alkaline sulphates, is the cause of this trouble. The fact of such great differences in the fusion of cones made from the same powder, and burned within 18 inches of each other, in the upper and lower muffles, and the marked effect of longer or shorter exposures to kiln gases, and the strange fact of the shortest exposure producing the worst swelling, all points towards the absorption of gas during the original fusion of the glasses, which is given out more or less completely during the long roasting which the cones receive in heating up before fusion. This physical structure of the cones is favorable to either absorption or evolution of gases, since the burning out of the dextrine leaves the mass spongy.

To test this theory, four glasses, 2-F, 2-G, 2-H, and 2-I, each containing

$$\left.\begin{array}{l}0.3\ Na_2O\\0.3\ K_2O\end{array}\right\}2.00\ SiO_2$$

in common, and containing respectively 0.4 equivalents of CaO, MgO, BaO and ZnO, and in addition, some Cremer frit whose formula was substantially

$$\left.\begin{array}{l}0.5\ Na_2O\\0.5\ CaO\end{array}\right\}\begin{array}{l}2.00\ SiO_2\\1.00\ B_2O_3\end{array}$$

were thoroughly ground to a fine dust; each was then mixed with an equal weight of pure Florida kaolin. The purpose of the kaolin was to make the mixture spongy, and thus hold the mass open for the absorption or expulsion of gaseous matter during the heating process. The kaolin would dry to a spongy mass, especially after it had been reduced in plasticity by the addition of 50 per cent. of non-plastic glass-dust, and after dehydration, the pore system would be still further developed by the loss of combined water and organic matter. The five different mixtures were then made plastic with water only, and molded into cones, of which each batch made seven.

When these cones had dried, three of each batch were placed in a small gas crucible furnace, about four and one-half inches internal diameter and six inches deep. A small pat of clay had been previously prepared with shallow triangular holes into which the base of the cones would fit, and thus stand erect. This pat had been previously fired, and was set in the center of the furnace, one inch above the floor. The furnace was then fired up, and heated until its walls had become thoroughly red hot, and at a temperature of 1100 or 1200 C. The gas was then turned off, the firing hole closed, the cones placed in the holes in the pat, the lid replaced and the cones allowed to stand for 20 minutes, in an atmosphere averaging 800°C at least, and free from combustion gases. No sulphuric acid or any other gas, other than air, could possibly have access to the cones during this short period of heating. At the end of 20 minutes exposure to full red heat, the cones were removed, cooled, pulverized and carefully analyzed*

*The analyses were by Mr. R. W. Nauss, B. Sc.

ANALYSES OF MIXTURES OF VARIOUS GLASSES AND KAOLIN HEATED IN 20 MINUTES TO REDNESS IN PURE AIR.

TABLE XX.

Elements.	Lime Glass.	Magnesia Glass	Barium Glass	Zinc Glass.	Cremer Frit.
SiO_2	59.40	62.10	51.54	56.32	
Al_2O_3	21.28	23.16	24.24	22.66	
CaO	7.30	0.68	.....	1.04	
MgO	0.07	2.96	0.36	0.20	
BaO	.....	.....	14.02	.....	
ZnO	.....	.....	.....	8.34	
K_2O	5.63	5.54	4.94	5.43	
Na_2O	5.01	4.83	4.13	4.40	
SO_3	0.32	0.20	0.55	0.21	0.082
Loss on Ignition	1.12	0.89	0.87	0.90	
Totals...	100.13	100.36	100.65	99.70	

Three cones of each of the five mixtures were then placed in the muffle kiln, on clay slabs as usual, except that they were exposed in the open kiln, and not protected by the usual covering.

They were brought up to a dull red heat at the rate of 100°C per hour, and were then subjected to a special sulphur treatment as follows:

The heat was advanced 100° at a time, but not by slow and steady gains. On the contrary, the heat was urged up as fast as possible, without flashing, occupying 20, 30 or 40 minutes to rise 100°C. The damper was then shut down tight, limiting the draft to what could escape by leakage. Into the fire, copperas was now thrown in successive doses of five and six ounces at a time, made up into a ball with enough plastic clay to keep it together, and to prevent the evolution of the SO_3 in a momentary cloud. This was continued until the heat had fallen back considerably, when the damper was lifted, and the temperature moved up another 100°C.

By this treatment, the cones were subjected to alternating periods of a still stagnant amosphere, reeking with SO_3 and combustion gases, and an actively circulating atmosphere charged with combustion gases and free air. No

period of reduction of any importance was permitted to occur, so that any SO_3 absorbed by the cones would not be expelled by subsequent treatment.

The copperas was used as follows:

TABLE XXI.

Time Periods.	Temperature Readings °C	Position of Damper.	Feeding of Copperas.
6:00 to 6:50 p. m.	510-610	Open	20 oz. in 4 portions.
6:50 to 7:00 p. m.	610-610	Shut	13 oz. in 2 portions.
7:00 to 7:30 p. m.	610-710	Open	12 oz. in 2 portions.
7:30 to 8:00 p. m.	710-710	Shut	24 oz. in 4 portions.
8:00 to 8:25 p. m.	710-850	Open	None.
8:25 to 9:10 p. m.	850-810	Shut	27 oz. in 5 portions.
9:10 to 10:15 p. m.	810-900	Open	None.
10:15 to 11:05 p. m.	900-875	Shut	42 oz. in 7 portions.
11:05 to 1:00 a. m.	875-1000	Open	None.
1:00 to 7:00 a. m.	1000-0	Shut	13 oz. in 7 portions.

Of the ten pounds of copperas used, about three pounds consisted of anhydrous SO_3. Also, it was calculated that the SO_3 and SO_2 rising from the combustion of the fuel itself, would amount to a very considerably quantity, probably 7.5 pounds, so that 10.5 pounds of SO_3 passed through the kiln during the burn. The chief advantage which was hoped for the use of the copperas, was the evolution of considerable quantities of SO_3 at the times when the draft was stopped; it was thought that that which passed through the kiln rapidly while the damper was up was little likely to strongly attack the glass-kaolin mixture.

Since the cones were liberally dosed with SO_3 at temperatures below the vitrification of their body, and while they were still porous and able to admit it, it was thought that if one glass had a less ability to withstand SO_3 or a greater tendency to lose any of its ingredients by volatilization, it should surely show it by analysis after this treatment. The final temperature reached was 1000°C, which was thought sufficient to assist in volatilization of alkalies, without risk of decomposing sulphates of the bases which might have formed.

On opening the kiln next day, we found the cones all vitrified to flesh-red, stony-hard, bodies, very much alike,

except the Cremer frit cones, which had fused and gone down at 950°C or about where cone 010 should fuse.

These cones were then finely pulverized and their respective contents of sulphuric acid (SO_3) determined.

TABLE XXII.

RESULTS OF SULPHUR TREATMENT.

	Percentage of SO_3 after heating in Pure Air.	Percentage of SO_3 after heating in Kiln Gases.	Increase in SO_3	Percentage increase on Original Amount.
Lime-glass Cone	0.32	0.47	0.15	47
Magnesia-glassCone	0.20	0.295	0.095	47
Barium-glass Cone	0.55	0.70	0.15	27
Zinc-glass Cone	0.213	0.247	0.034	16
Cremer Frit Cone	0.082	0.588	0.506	617

The results of this test are of great interest. The zinc-glass seems to contain as little sulphur as any glass in the lot, and to take up much less than the others, relatively and absolutely. The actual quantity of SO_3 taken up was only about one-third of that of the magnesia-glass, and less than one-fourth of the amounts absorbed by the lime- or barium-glass. The magnesia-glass shows a low actual sulphur content, but a rather high percentage rate of absorption. The actual amount absorbed, however, is small.

The lime- and barium-glasses show to a disadvantage from both points of view. They contain larger initial quantities of SO_3, and they absorbed larger quantities in the test. The lime-glass is unsuitable from this basis, then, in addition to the very clear evidences of unsuitability in the fusion trials.

The barium-glass has the advantage over the lime-glass of being more fusible, but its sulphur behavior is worse if anything.

The Cremer frit is the most surprising member of the series. Its extremely low sulphur content in the beginning and its remarkable absorption of SO_3 in the test explain the reason why this glass has given such unsatisfactory results in practice, as the flux for a cone series.

XI. The Relative Volatility of the Glasses.

It was thought that the alkalies would be the elements most readily expelled from the glass-kaolin cones, and that their loss would most rapidly affect the melting point of the glass. Accordingly, the same samples were carefully analyzed in duplicate for their alkalies, with the following results:

TABLE XXIII.

Mixture.	Heated 20 minutes to redness in pure air.			Heat 10 hours to redness in kiln-gases.			Loss or gain.
	K_2O	Na_2O	Total	K_2O	Na_2O	Total	Total
Lime-Glass Cone	5.63	5.01	10.64	5.73	5.18	10.91	+0.27
	5.96	5.18	11.14	5.58	4.78	10.36	—0.78
Magnesia-Glass Cone	5.54	4.83	10.37	5.53	5.15	10.68	+0.31
	5.63	4.50	10.13	5.91	5.16	11.07	+0.94
	.....	.....	.....	5.40	5.11	10.51	.....
Barium-Glass Cone	4.94	4.13	9.07	4.12	3.86	7.98	—1.09
	4.69	4.23	8.92	4.41	4.11	8.52	—0.40
Zinc-Glass Cone	5.43	4.40	9.83	5.54	4.86	10.40	+0.57
	.....	....	.....	.....	.....	.....	.....

It seems difficult to arrive at any clear conception from the above as to what actually takes place in these glasses. The analytical results are very probably somewhat at fault, as the duplicate determinations do not check as they should, in a number of cases. However, the analyses cannot possibly be so far wrong as to account for the main incongruities.

It is evident at least, that there is no characteristic or heavy loss of alkali, by the longer exposure to heat and acid fumes. In fact, the losses by silica and alumina must have been considerable in some cases to make the final content of alkali distinctly higher than in the beginning.

While these figures are not as conclusive as we should like, still they seem to make it reasonably safe to conclude that volatization of alkali is not a serious factor in any of the cases tested, and therefore, presumably, not with any other glasses which the further development of the series might require.

XII. Isolating the Most Favorable Glass.

Referring back to Section IX, we are now ready to attack it once more, and under better circumstance than in the first instance. We have shown, first, that zinc glasses, or magnesia glasses, or magnesia-zinc glasses are least liable to take up sulphur in offensive quantities and that volatility is not a serious fault with these glasses, more than with others.

Hence, the inquiry now narrows down to the consideration of what ratios of ZnO and MgO will give the most fusible and most stable glass.

NOTE.—The work had been carried but little further at the time of publication, and is not yet ready for announcement. It is hoped to make the selection of the best glass, and demonstrate a cone series made from it, in the future.

DISCUSSION.

Professor Charles F. Binns: I would like to ask Professor Orton to tell us what form of the salt he put into the frit in each case?

Professor Orton: I used soda ash, or common commercial carbonate to introduce my Na_2O. I used the laboratory carbonate of potash, but once our supply of the carbonate ran low and we used the nitrate instead. The calcium oxide was derived from pure whiting. The barium was used as a carbonate. The zinc was used as the oxide.

Professor Binns: Was there any connection between the use of the nitrate of potash and excessive boiling or foaming of the glass during fusion?

Professor Orton: I first had trouble from foaming when we used the nitrate, but later, when we used the carbonate, it boiled almost as bad. I was not able to see in the second burn that the carbonate of potash had behaved much better than the nitrate.

Professor Binns: Another question. Is it perfectly clear that all the CO_2 was expelled in the fritting? Is it perfectly clear that none of the swelling could be attributed to CO_2?

Professor Orton: No. I do not think it could. I don't believe you will find any CO_2 in silicates which have attained the condition of clear perfectly fused glasses. I don't be-

lieve it is possible that CO_2 could still remain in solution in the glass. Sodium carbonate decomposes pretty rapidly above 850°C, especially if mixed with silica. Heated to cone 8, as we did with these glasses, you get a perfectlv clear quiet glass, through which you can look down to the bottom of the crucible, as if it were water, instead of molten silicate.

Mr. Stanley G. Burt: I think it possible that all the CO_2 was not driven off in fritting. I have noticed in my experiments, when fritting an alkaline carbonate with silica, you first get an effervesence, and in time the melt gets perfectly clear and quiet, and you can go to an intense white heat without further change. You might imagine the carbonic acid was all gone, but drop the least additional silica in it, and more gas is at once set free. There must be some carbonic acid present to explain that. I would like to ask if you tried prolonged fusion in forming the original frits? I find that rapid fusion enables the surface to fuse, sealing it as it were, and destroying the posibility of the gas escaping. In slow fusion, long before the actual melting sets in, the bulk of the gases escape through the porous mass, and you can go ahead without swelling.

Professor Orton: The mixtures were fused for periods running from eight to twelve hours and were most carefully watched through the fritting stage, when the gases were most rapidly coming off. The mixtures in the cups would settle and shrink for a time, then begin to swell and rise up, and sometimes would extend like a cylindrical plug an inch or more above the edge of the cup. Then they would gradually flatten back, or the crust would cave in, showing the molten interior, and the whole would rapidly settle down to an inch or two of clean glass, as transparent as water.

Now, the evolution of this gas was such a distinct stage and its beginning and ending were so sharply marked that I have regarded it as a definite chemical reaction and have considered it as marking the expulsion of CO_2. The idea of a molten glass of any considerable acidity retaining any CO_2 in it after it has been heated 500°C above its point of visible gas evolution is new to me, and I am free to say it does not seem probable.

Professor Binns: That is a point which could be readily established. You could find it in the frit, if it was there.

Mr. Burt: You spoke of the fact that the cone should be mixed with 70 per cent. of frit, and 30 per cent. of the raw sterile mixture. Aren't these glass cones (pointing at samples) almost entirely frit?

Professor Orton: I did not say that a cone should contain 70 per cent. frit; I said in one case that one which contained 70 per cent. frit melted practically the same as if it were all frit. I said that 70 per cent. frit would not do; we would have to use a larger per cent. of sterile ingredients than 30 per cent.

Mr. Burt: How much sterile material would your best low-fusing frit carry, and still come down at 010?

Prosessor Orton: Fifty per cent. or a little more, I think. That is just a guess.

Mr. Burt: You intend to determine that in further work?

Professor Orton: Yes. I hope to carry this investigation through.

THE CO-EFFICIENT OF EXPANSION OF SILICA.

BY

STANLEY G. BURT, CINCINNATI, OHIO.

The student of Segers laws for body and glaze coefficient is at once struck by a seeming contradiction. He is told first that by increasing the percentage of silica in bodies, he increases his body coefficient, thereby reducing the liability to craze, and then further on he finds that leaving the body constant he may add silica to his glaze, thereby not increasing the glaze coefficient as might be supposed, but on the contrary reducing it. That is, the coefficient of the silica added to the body is greater than the average coefficient of the body ingredients. The final coefficient is therefore raised by adding it, while on the other hand the case is exactly reversed on adding it to a glaze.

I called attention to this in my paper two years ago, and offered as an explanation the fact that while in the body the silica was non-combined, that is free and crystaline, in the glaze it was combined and amorphous. Now while the coefficient of the crystaline silica is high, that of the amorphous is very low; hence we have the increase in the body coefficient, and the decrease in the glaze.

That crystaline silica has a high coefficient is well known. Probably the high expansion of a silica brick is the most striking proof. This coefficient has been accurately measured by Damour, who also measured the coefficient of various bodies showing that one containing 75 per cent. SiO_2 had more than twice the expansion of one showing but 68.4 per cent. So the increase in the body coefficient is readily understood, but what striking proof have we of its low coefficient in glazes?

This point was recalled to my mind on reading an article by Prof. Witte of Berlin, on what he calls an ideal glass. The article was really a discussion as to whether we should consider glass as a liquid or a solid, but the interest

in connection with my subject lay in his description of this wonderful new glass.

A glass, to be ideal, must satisfy many conditions, two of which have eluded all previous experimenters. These two are, *First*, that it have the lowest possible coefficient of expansion, and *Second*, the ability to withstand chemical action. We have all been maddened by breaking lamp chimneys and most of us know about the elaborate experiments to obtain a glass suitable for fine thermometers. The trouble in both cases is due to the high coefficient of expansion.

This expansion must be reduced to a minimum, but what is there from which we can make glass that will give us this low coefficient? The triumph of this discovery belongs to a German. And what does he take? Nothing more nor less than pure silica. He literally fuses pure silica and makes glass of it. Silica fused to a glass is in the amorphous form, and cools too rapidly to crystalize again.

The resulting glass is clear and fine, can be heated to intense white heat without softening, and when at this high heat may be thrown into ice-water without injuring it in the least, simply because of its low coefficient of expansion.

With this test in mind, we readily see how the silica being amorphous in the glaze, the more silica we add to, our glaze (that is, the closer we approach a pure silica glass) the lower our glaze coefficient will be. So Seger's seeming contradiction is explained.

Do these laws agree with the results our practical experience gives us?

Taking first the body, we know that fine grinding of the silica makes a great difference in the results obtained; the finer we grind, the greater the coefficient. However, this does not mean that we have altered the coefficient of the silica, but that we have enabled ourselves to get a better mechanical mix by fine grinding, thereby benefiting in the coefficient of the body as a whole.

In this connection, I may say that of two bodies showing the same percent of free silica, in one the silica merely added as such, while in the other body the clay as found in nature contained the free silica, the latter will have much the higher

coefficient. This, I believe to be due not only to the probable finer grain, but also to the better mix as just explained.

In firing, the body undergoes decided changes in the coefficient. We find that up to fairly complete vitrification, the higher we fire, the greater the coefficient. Mere addition of silica to the body will not raise this coefficient, but each addition of silica necessitates higher fire. That is, if a body maturing at cone 6 crazes, adding silica and still firing to cone 6 won't help matters, because your body is now under-fired. In this case, we consider the craze as being due to our body having too low a coefficient. Now as the body coefficient increases with the fire, an under-fired piece will leave too low a coefficient and craze. Adding flint to the body renders it less fusible, so in order not to have an under-fired body, the firing must be carried on to cone 7 or 8.

If on the other hand a body, which at the proper heat is free from craze, is over-fired, we may find craze setting in again. How does the theory of the two laws cover these practical results?

In the case of the over-fire, the explanation seems simple. Over-firing to the ordinary potter means vitrification of his body. Vitrification means converting the free crystaline silica into silicate formations, a change from high coefficient to low, which is just what we don't want in the body.

By the ordinary potter, I mean one making pottery as distinguished from one making porcelain. The porcelain maker aims for a complete conversion of his free silica and makes up for the resultant low coefficient in the body by using a very high percentage of silica in his glaze, and a consequent low glaze coefficient. My own experience shows the over-fire craze to be very rare.

Now, as for the under-fired ware, the explanation is not so simple. It does not seem probable that a substance without chemical change, would change its coefficient from mere heating. What we do know is that when we grind our silica, and thereby bring about a more intimate union, we greatly increase our coefficient. It seems probable that our increased coefficient from firing is due to the fact that as we

fire, the body contracts, thereby giving a more and more perfect union. That is, a semi-vitrified body will have a greater expansion than the same body, open and highly porous.

Before leaving the consideration of bodies, a point occurs to me which I wish some member fortunate enough to possess a fine microscope would investigate. This point refers to another one of Seger's laws. He tells us that substitution of a ball clay for a kaolin will increase the body coefficient. This is undoubtedly true, but why? Undoubtedly ball clays as a rule carry more free silica than kaolins, but even allowing for this, the result is the same. I believe it is due to the closer union of the particles that I have been speaking about. This closer union in a ball clay is due, first, to the fact that as it is a sedimentary clay, its particles have undergone a very fine natural grind in the waters which carried them, which the kaolin has not, and second, to the fact that the higher per cent of impurities in the ball clays cause it to contract much more readily. It is the question of the relative fineness of the ball clay and the kaolin, that I would like to see investigated with a microscope.

Considering glazes next, we find the same thing true that we found for bodies. That is, on adding silica, we must have additional fire. However, an under-fired glaze is as a rule so self evident, that no potter stops at an under-fired glaze.

It is at times hard to tell an under-fired body, but not as a rule an under-fired glaze.

To test the Seger laws, I made nine identical pieces and divided these into three groups of three each, calling them A, B and C. The A group was given an under-fire, the B a correct fire, and C an over-fire. All were then glazed in a uniform glaze. 1A was then given an under-fire for glost, 2A a correct fire and 3A an over-fire. The same scheme was followed in groups B and C. In this way the field was pretty wellc overed; that is, the field of over- and under-fireing.

On examining my results, the most noticable thing was that a decidedly worse result was obtained from an under-fired glaze than from an under-fired biscuit, though both were bad.

Another interesting point was that the under-fired glaze might have the clearness and brilliancy of one fully fired. That is, between many under-fired glazes, and the same fully fired, a change has taken place hardly noticable to the eye. If, however, we examine the under-fired glaze with a magnifying glass, we discover many small bubbles down in the glaze. The question at once arises if the glaze is fused to clearness, how to explain these bubbles?

If silica is added to a molten solution of Na_2CO_3, the CO_2 is displaced and sodium silicate formed, with the resultant solution less fusible. Now if we heat it higher, and add more SiO_2, more CO_2 will be given off. This we may repeat at a higher heat, and it is surprising at how high a heat CO_2 will still be given off. Each addition of SiO_2 renders the mass more infusible, and working with a blow pipe I was unable to reach a point at which further CO_2 was not given off.

Now in the case of $CaCO_3$ fired alone, we have no fusion and the bulk of the CO_2 passes off at red heat, but could not a considerable per cent of its CO_2 be retained if there was fusion? In making our glazes, we frit our Na_2CO_3 and $CaCO_3$ together with ingredients, such as $B_2(OH)_6$ and PbO, both low fusers, thereby I believe retaining considerable CO_2.

Now I believe the bubbles I spoke of are CO_2. Some claim they are air bubbles, but this cannot be, as you can see them over our vitrified bodies. They are not the miserable SO_2, of which I have spoken before, as these leave no blister after them, merely small not unpleasant pin holes; so I feel sure these are not SO_2.

The fact that the under-fired glaze appeared clear would seem to indicate that the SiO_2 in the glaze mix had gone into a silicate formation. By clear, I mean there was nothing undissolved in suspension. The bubbles of CO_2 still present indicate to my mind the fact that at the heat at which the glaze was fired, there was not sufficient SiO_2 present to displace all the CO_2 which could be displaced at that heat. So the glaze, acting on the body, is dissolving SiO_2 from the body which in turn is displacing CO_2 from the glaze. If now the glaze is held sufficiently long at a uniform heat, we will

presently reach a point at which no more can be displaced for that heat. This is due to the fact that taking up SiO_2 from the body renders the glaze less fusible, therefore less fluid; the movement of the molecules is retarded, and any carbonate directly in contact with the body being decomposed, no fresh carbonate can reach it. If however, the heat be raised, the reaction starts up again, and fresh bubbles are formed, So to free a glaze from these bubbles, it should be held constant at its maximum heat a sufficient length of time. This is just what the majority of potters do. I do not claim that all glazes would noticably show this phenomenon. Many have too low a percentage of carbonate in any form, but I believe the majority do. I have known glazes that could be freed from this defect in no other manner.

A glost kiln cooled too rapidly shows these pin hole pits very badly, due not only to the scars not having had time to heal, but to the fact that slow cooling gives more time for the glaze to free itself as I have explained.

It may seem hard to see just what connection all this has with my subject, but what I aim to show is that while the under-fired glaze crazed, and the correctly fired one did not, this was due to the latter taking up silica from the body. That the glaze so badly under-fired as to have parts unfused should craze so very badly, would indicate that here the silica had as yet not entirely changed to the amorphous form.

At some future time, I hope to speak on the seeming error of what I may call the silica laws when applied to matt glazes.

DISCUSSION.

Mr. Thomas Gray: I would like to ask the cause of over-fired wares crazing.

Mr. Burt: The damage from over-firing is in the body, not the glaze. We want to get the high coefficient of crystalline silica. Over-firing means the conversion of crystalline silica into silicate formations. It is no longer crystalline silica with a high coefficient, but it is an amorphous silicate with a low coefficient.

Mr. Gray: I want to inquire what the statement as to too rapid cooling in the glost kiln means? We draw the fires as soon as done burning, and soft coal cools more rapidly than hard coal. Do you mean cooling more rapidly than with soft coal? I have often wondered whether the pimple indentations were formed by too rapid cooling.

Mr. Burt: There are a great many methods of cooling a kiln. The porcelain manufacturer throws the kiln wide open, and lets in cold air with a rush. That is rapid cooling. In the ordinary pottery works, they seal the glost kiln up. Sometime ago Professor Binns wrote me and asked me what my opinion was as to these pin holes, and I told him what I do you. He asked me what was the remedy, and I said not to cool too rapidly. He went a step further than that, and did not draw his fires, cooled slowly, and was at once freed from the trouble. However, you have got to be careful not to generate a reducing gas by leaving the fires undrawn and sealing up a kiln.

At the Cleveland summer meeting, we took a tour down to Akron; a very interesting trip. As we were going through a stoneware works, they had just fired off a kiln, and a man was sealing up the kiln. Professor Binns asked him what he was sealing that kiln for, and the man answered that if he did not seal the kiln, he would get ware full of pin holes.

Mr. Gray: As soon as we are done firing, we pull the fires right out and sometimes open the test holes half an hour after the fires are drawn. I have not noticed any pimples.

Mr. Burt: What I say in my paper is, that I do not claim all glazes suffer from this difficulty. If your glaze does not happen to suffer, cool as rapidly as you can, but if one of you is unfortunate enough to suffer from this difficulty, then I say your remedy is to cool slowly.

The Chair: I think in view of Mr. Gray's former remarks about matt finish, he might try cooling slowly. Whether that matt surface is pimple or not, will take a microscope to determine.

THE CONSTRUCTION AND EQUIPMENT OF A WHITE WARE POTTERY.

BY

ARTHUR S. WATTS, NEW LEXINGTON, O.

For the past year I have devoted my entire time to the designing and erection of a 12 kiln china ware plant. From my experience along this line, I propose to offer the society my idea of a white ware plant, trusting that it may find some helpful suggestions from a study of my ideas. As I shall publish an outline plan of the buildings, it will be unnecessary to discuss their arrangement here. What I propose is to take up the various defects common in such plants, and offer what I propose as a correction.

While I approve of all practical labor-saving devices, I do not consider any mechanical device a good investment that will not save its cost in one year. We will first look to the power plant, which I will discuss in detail later. In my engine room, will be two engines and two dynamos. One of the latter will be used for power, and the other for light. In the average white ware plant, so little power is used beyond the clay shops, that none is supplied beyond this point. I propose to make use of motors wherever power is needed beyond the bisque kiln shed, as it is quite as economical and saves the long shafting otherwise necessary.

Adjoining my engine room, I propose a thoroughly equipped machine shop, where all work of the factory may be done.

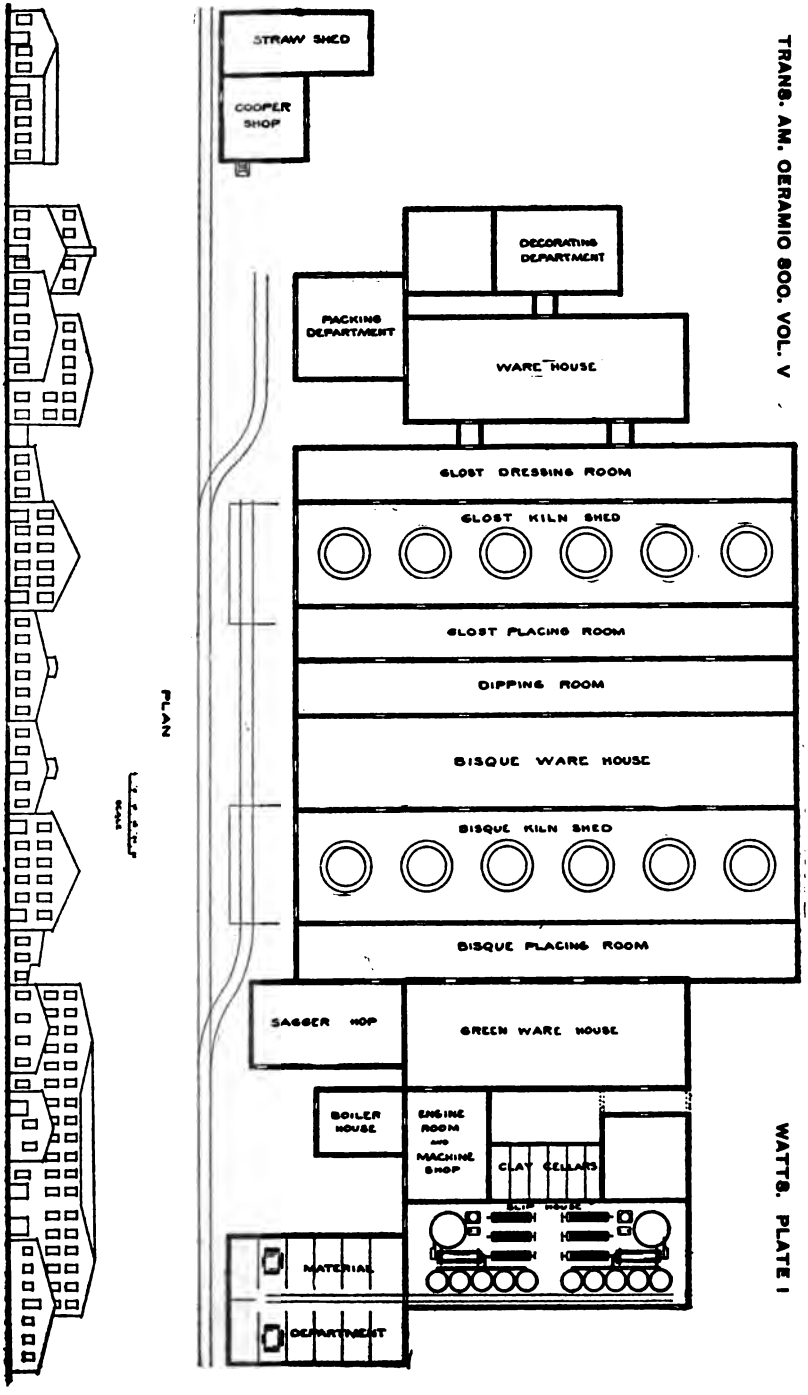
In the clay rooms, the first subject that demands our attention is the material and clay sheds. Many factories have these sheds so located that a long haul is necessary to reach the slip house. I propose to make use of modern conveying machinery, and with a grain shovel inside of the car, I will be enabled to unload a car of material with one man in one-third the ordinary time. I also find that flint

sand can be purchased for about $1.00 per ton, and even with the expense of installing tube or ball mills for grinding, the ground flint can be produced for much less than the market price. The same is the case with feldspar rock. What I propose is to install porcelain-lined tube mills and grind my own flint and spar.

The next practice that I propose to improve upon is the preparation of the body. We must have the body uniformly mixed, if our ware is to be of a uniform color and appearance. The flint and spar ordinarily used are materially coarser than the clay, and hence, especial care must be taken to have them thoroughly mixed. This can not be done in the old fashioned blunger. What I propose is a series of blungers, from which the body passes to a tube mill such as is commonly used for grinding Portland cement. This is the best rapid-grinding apparatus known for clay working, and by careful adjustment the body can be brought from this mill in a perfectly uniform and very fine condition. It has been my experience that this is the only successful means of overcoming cloudiness so common in American porcelain.

From the tube mill, my slip will pass through a trough, where I have installed a battery of horseshoe magnets, charged from my electric lighting line. By this method, I remove any particles of metallic iron that may have stolen into the mixture. I find this a very good thing, since about any factory, annoyance from a few specks of iron in a fine piece of ware is no uncommon occurrence.

The body then passes through the sceern into the deep agitator. I will here explain what I consider an ideal clay slip screen. It is nothing more than a rectangular box 24x30 inches, made of heavy wood to stand jarring. It is set with one end raised about 10°, and the upper end is left open to permit the workings to pass out. The slip is introduced at the lower end. The shaking is produced by having the box supported on four straight steel springs, two at each end. A heavy cog wheel forces the box out of


STRAW SHED
COOPER SHOP
DECORATING DEPARTMENT
PACKING DEPARTMENT
WARE HOUSE
GLOST DRESSING ROOM
GLOST KILN SHED
GLOST PLACING ROOM
DIPPING ROOM
BISQUE WARE HOUSE
BISQUE KILN SHED
BISQUE PLACING ROOM
SAGGER SHOP
GREEN WARE HOUSE
BOILER HOUSE
ENGINE ROOM AND MACHINE SHOP
CLAY CELLARS
SLIP HOUSE
MATERIAL
DEPARTMENT
PLAN
ELEVATION

position until a cog can pass under, when the box flies back to its original position, striking either the next cog or a block set for the purpose. This produces a very strong jar, and frees all collected refuse from the surface of the screen, jarring it up the incline until it passes over the top. I have seen this type of screen work for months without needing any repair, and it possesses the advantage of requiring very little height, whereas, the ordinary screen requires from two to three feet of height, necessitating the placing of all blungers very high, and requiring a proportionately higher building.

From the deep agitator the slip goes to the press by means of the pump. If any one machine in a pottery may be considered the "potter's burden," it is the slip pump. Every potter has his own idea of a pump, but mine is a rubber-ball-valve pump. This works by a plunger, but instead of the ordinary brass or leather valve, it has a socket into which fits a rubber ball. This can not possibly get out of the cavity, and when it becomes worn too small, it can be replaced for a few cents, thus giving you practically a new valve. As the rubber takes all the wear, the socket lasts for years This pump gives less trouble where used than any that I have ever tried or seen tried, besides being cheaper than the brass valve commonly used.

Another idea that I have proven to be thoroughly practical, is a pad under each sack on the press. Any slip-house man will tell you that it takes longer to put the last quarter of clay into a leaf than is consumed in pumping up the first three-quarters of the leaf. This is owing to the canvass sack being pressed tight against the iron leaf of the press, leaving no room for the water to run out. By placing a layer of coarse felt or canvas between the sack and the iron leaf, the water can soak out at any ordinary pressure as fast as the pump can force it in. This enables you to pump your press up to 100 pounds without stopping, and you will find that it will take less than one-half the time consumed by the old single sack method. I find further, that a press thus equipped will not burst and run, as the clay

from the broken sack meets the cushion of felt or canvas and clogs, thus enabling the press-man to continue with his pumping, and perhaps use the broken sack the remainder of the day without any trouble. I also find this a great saving in sacks, as no loss is experienced from wear against the iron leaf.

From the press, the clay goes to the cellar. Here is the weak point in most white ware potteries, especially china plants. Ample room must be supplied for aging the body, as otherwise much loss is suffered from cracking. Most clay cellars are too cold. This is the worst condition possible where a bacterial growth is desired, as is the case here. The temperature of a clay cellar should never run below 76°, and an abundance of moisture should be provided. This condition I propose to insure by placing steam pipes next my ceiling. My walls will be lined with heavy oak plank, as will also the floors. The cellar will be of sufficient size that the clay may be aged for two weeks. I find that under proper conditions, a longer period of aging proves detrimental since the clay becomes rotten and full of tiny gas bubbles, which enlarge on burning and disfigure the ware.

The clay shops are on a level with the other floors of the factory, the additional height required being supplied above. This brings the main power shafts on a level with the power shafts of the second floors, except in the room above the slip room, and reduces the amount of belting necessary, while it insures a good dry floor for your slip house. The elevated floor above the slip house is used for a casting room, and no power is required, except for pumping up slip, which is done from below.

The glaze is all ground in ball mills and pumped across to the dipping rooms.

The clay, after aging, is pugged, and goes by means of a belt elevator to the various departments above. The work shop occupies the second floor, and is equipped with oval jollies, etc., reducing hand labor to the minimum. The ware, after being inspected, goes to the green room on the

first floor. This room is divided into four sections; heavy-flat, thin-flat, jollied-hollow, and cast-hollow, enabling the placer to find what he wants without any trouble or delay.

We now come to the placing of the green ware for the bisque kilns. Here the handling of saggers and placing material confronts us. It is customary to place extra saggers about the kiln-shed when not in use, but to this practice I lay much of our dirty ware. It is certain that with a clean, light place for the saggers when not in use, and, where the placer may fill the sagger, a much neater and more satisfactory result will be obtained than in a dirty, smoky kiln-shed. I therefor propose a 25 foot room, between the green wareroom and the bisque kiln-shed, where saggers may be stored and filled.

To the subject of kilns, I have given some attention, and while I have not been able to convert others to my views, I am convinced that the center stack down-draft kiln is the ideal for white ware, as well as for tile. I can not see the economy or necessity of retaining the old up-draft kiln. Any one who has studied the workings of an up-draft white ware kiln, knows that the outside ring is solid-flat ware, laid in flint or sand, while the center of the kiln consists of hollow ware. Between this and the center, is a great area that is too soft for flat ware, and too hard for hollow ware. The ware taken from this area is never right. Why can we not use a center-stack down-draft kiln, which will give uniform results, and burn only such ware in a kiln as can be all burned at the same temperature? This would necessitate setting an entire kiln of hollow, for every two kilns of flat, but this could be done, as it would mean two kilns of hollow ware, and four kilns of flat ware each week, in a 12 kiln plant. The hollow ware could be burned at a lower cone than the solid-flat ware, and this would produce a product uniform in color and hardness.

The bisque ware goes by a gravity conveyor to the bisque warehouse. Here, I would use power to assist me in removing the sand or flint from the ware. For cleaning all pieces less than three and one-half inches in diameter, such

as creams, butter slips, and small heavy sauce dishes, I use a hickory-lined ball mill filled with sand. The ware is placed in the mill and sand added until it is nearly full. The mill is run until the dishes are clean. The large round ware is cleaned on a wheel. The irregular shapes are cleaned by hand. As soon as the ware is cleaned, it is dusted and placed in tight boxes on wheels, in which it goes to the dipping room adjoining. As the ware is glazed, it is placed in a dryer with doors at both sides, enabling the glost kiln placer to reach the ware without entering the glazing room. This dryer also confines the heat to the glazed ware, and makes a much more agreeable dipping room than we generally find. From the glost kiln, which is also a center-stack down-draft, the ware goes to the cleaning and sorting room, where it is carefully sorted, and such ware as must be decorated, in order to be made saleable, is taken to the decorating warehouse, while the first and second-class ware goes to the glost warehouse. Ware should not be decorated until ordered, and then go directly from the decorating kilns to the packing department, where it is to be wrapped and packed.

Crates and casks for shipping ware will be made on the premises, and brought by gravity conveyor directly into the packing room. Straw for packing will be handled by the same method, hence no quantity of packing material need accumulate in the packing rooms where it might cause a disastrous fire.

Coal for my kilns will be run on an elevated switch, and bottom dump cars being used, no expense will be incurred in unloading. The coal will be distributed about the kilns by means of cars running on overhead tracks and so arranged that the loaded car carries the empty car back.

The sagger shop is so located that the carry to the bisque kilns is the shortest possible.

It has been suggested that my ware houses are small, but actual experience has proven that they are ample to carry a $150,000.00 stock, and I consider that any concern

which accumulates more than that amount of stock must have some cause for being unable to dispose of their product, and should suspend operation until their stock is reduced or the cause of unsalability overcome.

In closing, I desire to remind you that these are simply suggestions, and while they have proven practical wherever I have seen them used, they are open to criticism and would doubtless require changing to suit the location and existing conditions. For a level site and unlimited space, however, I feel that I can assure you that a plant of this description and plan would enable the owner to produce ware at a much lower cost and of a better quality than much that is produced today.

IMPROVEMENTS IN APPARATUS FOR CLAY ANALYSIS.*

BY

EDWARD S. BABCOCK, M. SC. AND CHARLES F. BINNS, M. SC.,
ALFRED, N. Y.

Sampies of clay being sent in large numbers to the New York State School at Alfred, for analysis, it has become necessary to devise special apparatus to facilitate the work.

For the preliminary fusion with alkaline carbonates, the blast lamp presents many disadvantages. If worked by foot power it is irregular and exacting, with the frequent result of imperfect decomposition, while a power blower is not always available.

The following is a description of a simple, satisfactory piece of apparatus:

A covered box of sheet iron is prepared, measuring ten inches long by eight inches wide and eight inches high. Three apertures are cut, two in the top measuring two inches and five inches in diameter respectively, and one, two inches in diameter, in the end farther from the larger aperture. All three openings are circular.

The box is filled with plastic sagger clay well mixed with grog. The clay is then excavated horizontally from the lower hole and vertically from the two upper holes in such a manner as to form a tunnel which returns upon itself as shown in figure 1.

A thin vessel of fire-clay is made and burned. The shape is that of a thimble with a broad flange. This is dropped into the upper opening and should hang freely with a space of about one-fourth inch all round. This is shown in position for use in figure 1.

A Bunsen burner is placed opposite the lower hole and a length of stove pipe over the small upper one. For a burner, the bunsen of a Welsbach light answers well, but the orifice should be enlarged to $\frac{1}{16}$ inch.

*Read by title at Boston meeting. Discussion specially invited.

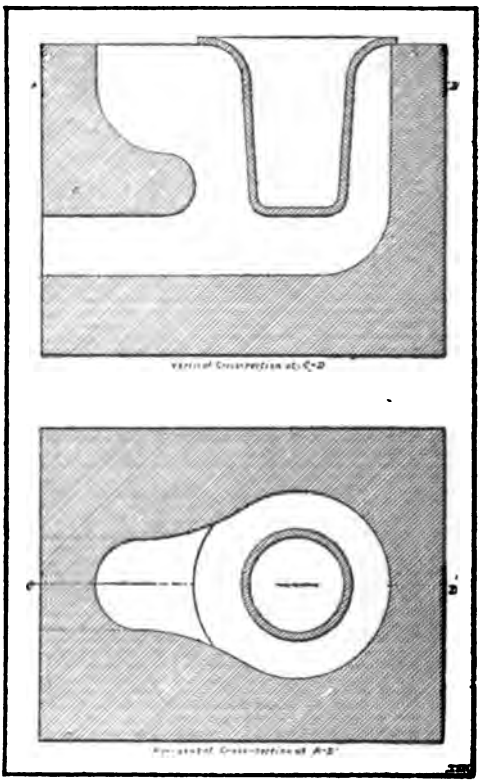


Figure 1.

The platinum crucible with the alkaline mix is set in the clay thimble and will need no attention after the lighting of the gas until a perfect fusion has been secured. Inspection is easy by reason of the wide opening above.

Everyone who has used platinum is aware of the trouble caused by reduction. Particles of metal from surrounding objects, metallic dust from the blower and lamp cling to the platinum and are assimilated, to the speedy destruction of the ware. All this is avoided in the apparatus described, and it is every bit home-made.

A second furnace has also been constructed for the subsidiary operation of alkali determination. Two points in this work are always giving trouble to the analyst, first, volatilization and second, imperfect decomposition.

The method employed is that involving the breaking

down of a clay by ignition with calcium carbonate, known as the J. Lawrence Smith method. A special platinum capsule was procured, measuring 8 centimetres long and 1¾ centimetres wide at the top and 1½ centimetres wide at the bottom. This is fitted with a cover. The furnace is similar to that previously described except, that in place of the large opening at the top, there is a small one in the side below. See figure 2.

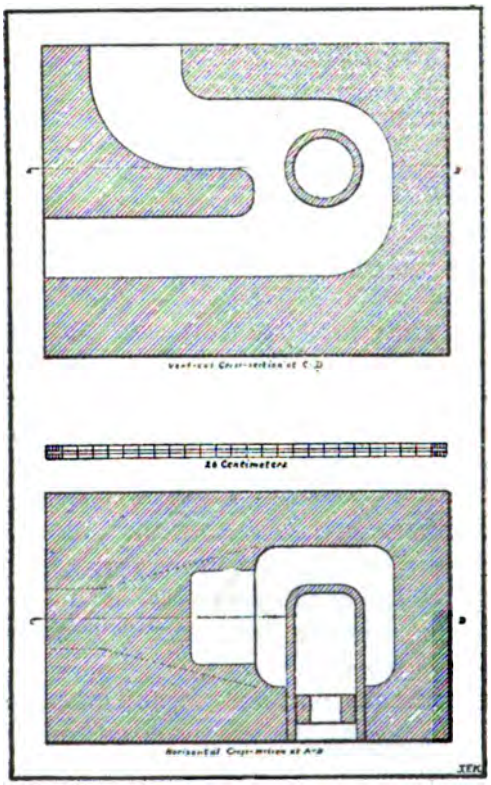


Figure 2.

In this hole a fire-clay tube is inserted, and in the tube the platinum capsule with the calcium mix is set. A long period of ignition can be employed. The cover of the capsule prevents volatilization, and owing to the length of the platinum vessel, the upper end keeps cool enough to condense any vapors without loss.

BACTERIA IN CLAY MIXTURES.

BY

EDWARD C. STOVER, TRENTON, N. J.

For the purpose of testing bacterial inoculation, on Dec. 15, a short vitreous body was mixed, having the following composition:

$$\left.\begin{matrix}0.130\ K_2O\\0.052\ CaO\end{matrix}\right\}\ 1.00\ Al_2O_3\ \left\{\ 6.26\ SiO_2\right.$$

which figures out as follows:

Clay, 1200,
Flint, 1200,
Spar, 400,
Whiting, 28.

The quantity put up was divided into two equal parts, one of which was mixed with bacteria-laden water, the other with fresh pure water, and both set aside for one month until Jan. 15, when both batches were given to a presser with instructions to make a small baker of each.

The two batches differed much in appearance, the pure one remaining light in color, while the inoculated batch had become quite dark. The latter was tough and plastic and worked without difficulty, while the former was so short it was almost impossible to work it. We could hardly make the presser believe they were both of the same mixture. He declared that the dark colored tough batch was either a different mixture altogether, or had something put into it to make it work better.

This mixture was purposely made without the use of any ball clay to assure its being a short body. Both samples were fired at cone 13, and a glance at them will show clearly which is which, the one made of the inoculated mixture showing a comparatively smooth surface, while the other is full of small rends or tears common to very short bodies.

Both of these bakers were made from the same mould and a slight variation in shrinkage is to be noted, the inoculated mixture showing the greatest shrinkage, which seems

to prove the theory of a closer union of the minute particles in the inoculated mixture than in the one not inoculated.

Potters who are manufacturing porcelain, bone china and other bodies of a short non-plastic nature, could probably make some experiments along this line to their advantage.

DISCUSSION.

Professor Edward Orton Jr.: I will ask Mr. Stover how he prepared the fresh, pure water, and how he knows it was free from bacteria?

Mr. Stover: I took ordinary hydrant water, assuming that is as pure as we could get around any pottery. The foul water was made from a batch of clay, which had been standing for about three months,and was very offensive. The water used for inoculation was a decantation of that mixture. It was very black and very offensive when stirred up. I could guarantee the impurity of that, rather than the purity of the other. In some districts the mixtures of clay are made nine months or a year before making up into ware. The clay becomes quite dark and offensive; and my theory is that it is a bacterial growth which produces this condition.

Professor Orton: Is there anything put into the bodies for bacterial food?

Mr Stover: Nothing, whatever.

Professor Orton: What would you think of putting in a small quantity of starch paste or sugar waste or something of that sort, which would act as a ferment, thus affording food for the bacteria?

Mr. Stover: I think it is a good suggestion. The thing to do is to find out the bacteria which are doing the work and then regularly cultivate them in our mixtures.

Professor Orton: It seems to me probable that the bacterial form which is producing this fermentation and decay is an exceedingly common form, the presence of which we could hardly avoid. It does not seem to me so important to enter into an elaborate microscopic study of the exact species. It seems more important to furnish a proper me-

dium in which they can work. A half or quarter per cent. of decomposible organic matter, would make an enormous difference in the rapidity with which they breed. Ordinary clay does not contain very much food for bacteria.

Mr. Stover: I think the suggestion is a good one, and I will experiment along that line. My object is to reach quickly the same result now obtained by aging clay. Potters generally realize that clay put in a cellar and left awhile, is tougher than when fresh from the bank. Why take two or three months for this result, if it can be reached in three weeks? I will go on along the line suggested, and get further results if possible. It has been shown by others that bacteria flourish best at one hundred degrees Fahrenheit or about that point.

Mr. R. C. Purdy: Is bacterial growth accelerated in a room lined with oak, rather than in a cellar lined with cement?

Mr. Stover: I cannot say. I have not been lining any rooms for the purpose. I have been told that in the manufacture of certain cheeses, which our German friends are very fond of, they must be aged in certain places to get the right result, probably for the proper cultivation of the bacteria.

Professor C. W. Parmelee: Professor Orton suggests the advantage of continuing these experiments along the line of inoculation rather than pure culture. Pure culture might have the advantage of dispensing with the offensive odor Mr. Stover speaks of.

Professor Orton: Mr. Stover's paper last year contained a reference to certain bacilli from one of these cultures, and if I recollect correctly he gave it as Bacillus Sulphureum. It seemed to be one which would be apt to be found in decomposing clay mixtures particularly. They contain more or less pyrites, or other sulphur-bearing ingredients, and these are broken down by the bacteria and sent out as H_2S, which generates a noisome smell. It seems to me we cannot expect to accomplish the work without smell.

Mr. Thomas Gray: I will ask Mr. Stover if he made up

any large amount of these clays (referring to samples accompanying Mr. Stover's note), to know which one had the most loss.

Mr. Stover: No; only these two samples.

Mr. Gray: One, I notice, shrunk in width, and they are about the same length.

Mr. Stover: If broken and placed under the microscope, I think the one inoculated will show somewhat tighter, closer structure than the other.

Mr. Gray: We had some clay which had been mixed for six months, and the man who worked on it said he never had such tough clay and could handle it much better than the freshly mixed clay. I never noticed anything disagreeable about it myself, and did not hear him complain. In handling it, the clay which had aged for six months was very free from cracking.

Mr. Stover: That seems to be along the line of experience of others.

NOTE ON THE FLUXING POWER OF MICA.*

DISCUSSION BY PROFESSOR HEINRICH RIES, PHD.†
ITHICA, N. Y.

Mr. Stull's paper on the fluxing effect of mica in ceramic bodies is one that I am sure interests all of us. Mr. Stull comments on the possible reason for muscovite being the common mica in white clays as due to the fact that it closely resembles feldspar in its chemical composition. If we include sedimentary clays in this state, it would seem to me that the absence of the other micas, especially the iron bearing ones, might be because they are so easily decomposed when the clay is attacked by the weather, and therefore in the case of sedimentary deposits it is the more resistant minerals that are left. In the case of residual clays his explanation may be true.

Mr. Stull's results certainly bring out very prettily the effect of fineness on the fluxing power of different ingredients. The larger grains of mica such as are visible to the naked eye in many clays may seem to resist union with the other ingredients of the clay up to a moderately high temperature. I happen to have on my desk at present a whitish clay from New Jersey, which contains a large percentage of white mica particles, any of which would however pass through a 100 mesh sieve. After heating a bricklet of this clay up to cone 8, many of the scales were still easily seen in the middle of the bricklet, although they have lost some of their lustre. At cone 10, those in the interior of the bricklet had nearly all disappeared, although they were still visible on the outside of the bricklet.

The effect of the size of the grain on the fusibility of the clay is a point which has always interested me considerably, and I have recently begun some experiments with different common minerals such as are apt to occur in clay, in order

* Transactions American Ceramic Society, Vol. IV, p. 255.
† Read by title at the Boston meeting, February, 1908.

to satisfy myself on this point, and to find out what fluxing influence the different minerals exert. The common supposition has been that the smaller the fluxing grains, the easier the fusibility of the clay, although in the case of fire clays, Prof. H. O. Hofman considers that this is not the case.

As a first step I made up some mixtures of kaolin with hornblende and kaolin with calcite.

One series contained by weight as follows:

A. Kaolin 25 parts, calcite 25 parts, the latter passing through 150 mesh.

B. Same mixture, the calcite passing through 100 mesh, but resting on 150 mesh.

C. Same as before, but calcite smaller than 80, and larger than 100 mesh.

The object of taking such a large quantity of flux was to have the material melt at a temperature below cone 10 if possible.

A similar series of kaolin and hornblende was mixed up, and these can be designated as A′, B′ and C′.

The results were not unexpected.

At cone 10, A had fused, but not run, B was softening, while C was unaffected.

In the case of the second series similar results were obtained at cone 5.

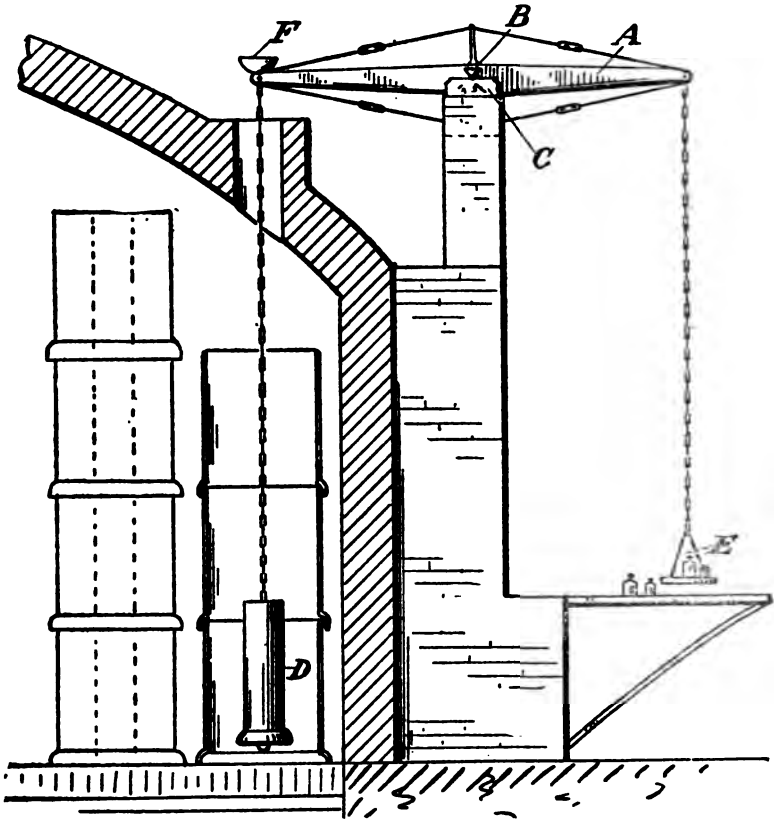
This is simply a preliminary note, and I hope to present some detailed results to the society at the next meeting, after I have carried out the experiments with a number of minerals as well as mixtures.

A GRAPHIC METHOD OF SHOWING LOSS OF WATER IN CLAY BURNING.

BY

WILLIAM H. GORSLINE, ROCHESTER N. Y.

In common with others, we have experienced the disasters attending the too rapid firing of our ware when green, or before the water-smoking process is completed. The idea which led to the adoption of the apparatus employed came from the suggestion of a friend. To describe it would be to compare it to the beam of a chemical balance (see cut.)



But one knife edge (b) is used. This passes through the beam (a) near the center of gravety and extends through each side. On each side of the beam are blocks (c) which rest on the base on the top of the kiln. On the top of these blocks are iron plates to receive the knife edges. The beam is thus allowed to act freely and responds to very slight differences in weight. Suspended at one end, on a line with the knife-edge, is a wire which passes through a hole in a kiln cover. From this wire is hung a trial piece (d) near the floor of the kiln. This trial piece is of the same thickness and degree of dryness as the ware which predominates in the kiln. From a point on the same line at the other end of the beam is hung a counter-weight (e) which is sufficient to bring the beam into equilibrium before the firing begins.

The beam is braced at top and two sides by small cables and turnbuckles. It is made of very dry clear pine instead of metal. For a knife edge, I used an old file ground down to a sharp edge. The foregoing is a brief description of the apparatus used. I have added to it other improvements which it would be difficult to describe in this note, but which would suggest themselves to any one familiar with such work.

As the firing begins and the water-smoking progresses, the mechanical water is expelled from the ware, thus making the latter lighter and causing the end of the beam from which it is hung to rise. Observations are made at intervals, when enough weights are added to a receptical (f) at the end of the beam just above the kiln cover, to restore it to balance.

As is quite clear, the total weights added at any time equal the total weight of the water expelled. Although the results ultimately possible have not yet been secured from these experiments, the point at which the mechanical water has left and the chemical water begins to be expelled, is roughly indicated. The time during which the chemical water is being expelled is unmistakable, on account of the pronounced loss of weight indicated by the apparatus.

In the firing of sewer pipe, we consider this stage of the burning one of the most critical. In fact, as regards the

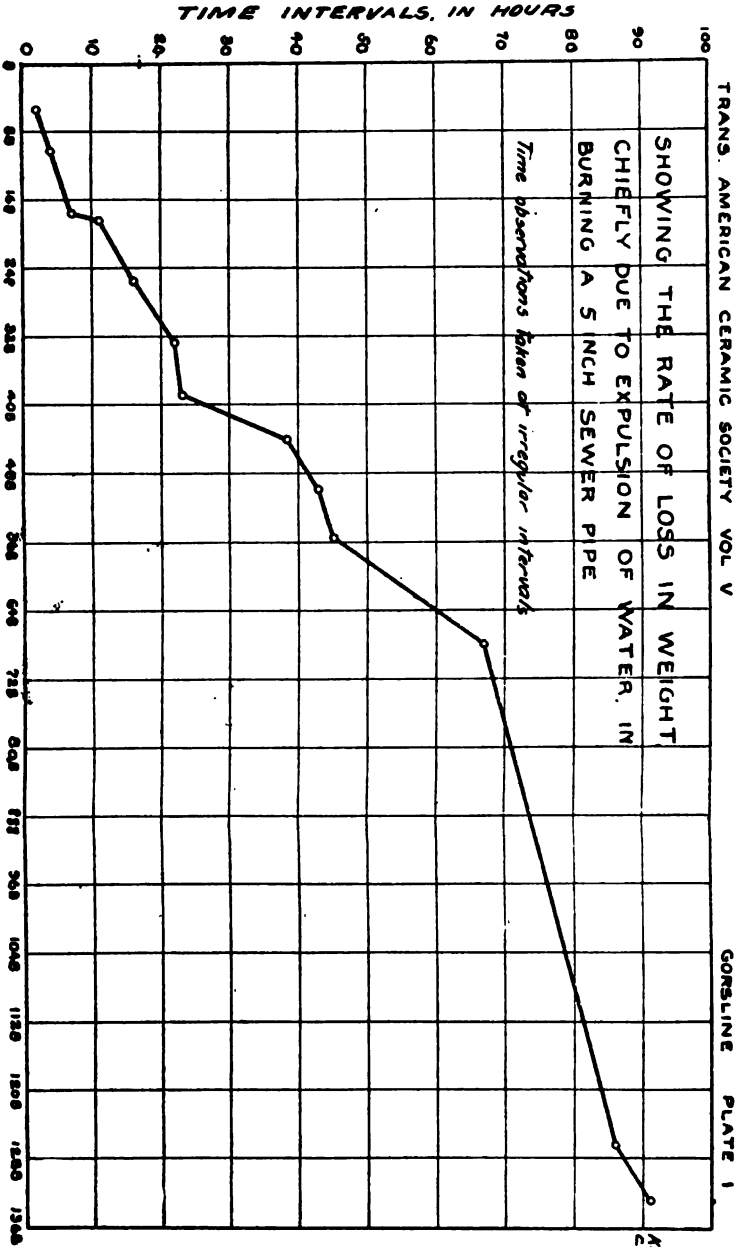
water, it *is* the most critical. It is not common to hear from those burning finer wares, the complaint that when the fire is increased before the ware is sufficiently dry, the ware is injured or ruined by the too rapid expulsion of the water. It may be possible therefore to adapt the principle described above to the burning of fine ware, so that the behavior of the beam may regulate the first part of the burning.

In one experiment, extending from the begining of a firing which occured on a Thursday at 6 P. M. until the kiln was entirely clear on the following Monday at 2:15 P. M., the entire loss of water in a dry five-inch pipe amounted to 1330 grammes. During the first 68 hours, which was up to the beginning of redness, there was a loss of 681 grammes; during the next 24 hours, a loss of 649 grammes, when the kiln was clear. It is thus shown that during the first period the average loss was a little over 10 grammes per hour, during the second period 27 grammes per hour. The difference in favor of the latter, indicates that the chemical water was being expelled during that period. Plate I shows the curve described by such a burn. In another, during 60 hours, the loss was 430. In the first 43 hours to redness, loss 223, in next 17 hours to beginning of clearness, loss 205.

In our regular burning, we use cones one, two, three and four, but number one does not go down until the last day, and, on account of lack of time, in none of these experiments have I taken the temperatures. Nor have we been able, so far, to continue the observations beyond clearness because after that the wires that we used would not withstand the high temperature. It would be possible, however, to continue this measuring of the water expelled, until the beam remained constant, although that might not be reached until the kiln came off.

The total loss indicated is not actually all water, as other losses cccur, viz. SO_3, CO_2 etc; but these are not so important as regards weight.

In chart No. 2, the curve is interesting for several reasons. The burners are changed at 6 P. M. and 7 A. M. This curve shows a marked loss beginning soon after the changing of burners one evening. Of course, it is somewhat


TRANS. AMERICAN CERAMIC SOCIETY VOL V
GORSLINE PLATE I
SHOWING THE RATE OF LOSS IN WEIGHT CHIEFLY DUE TO EXPULSION OF WATER, IN BURNING A 5 INCH SEWER PIPE
Time observations taken at irregular intervals
TIME INTERVALS, IN HOURS
0 10 20 30 40 50 60 70 80 90 100
LOSSES OF WEIGHT IN GRAMS
0 80 160 240 320 400 480 560 640 720 800 880 960 1040 1120 1200 1280 1360
KILN CLEAR RED

due to the progress of the firing, but we are confident that it indicated some carelessness on the part of the burner. The irregularity of the curves is, of course, due to the factory conditions, which are not as even and regular as though the burning were conducted in a laboratory.

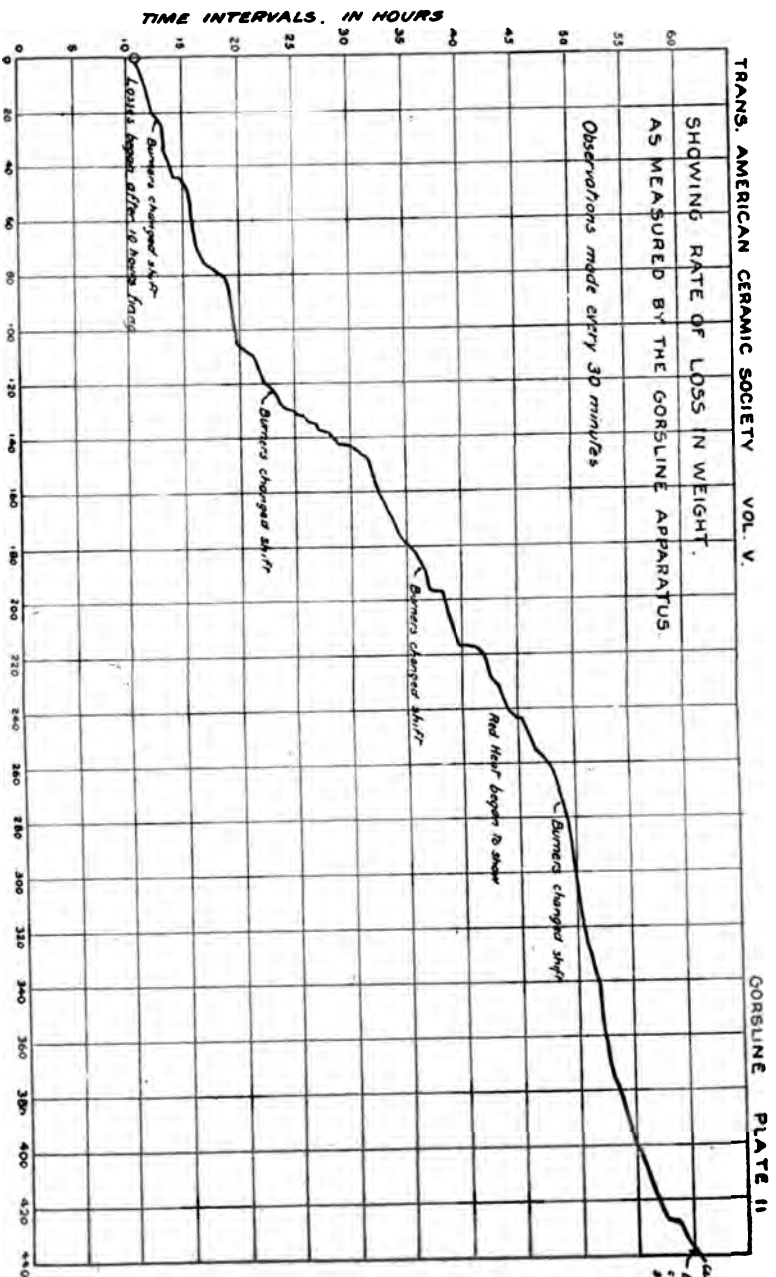
We think this apparatus will help us somewhat in perfecting the conditions in our own factory in regard to this one point.

DISCUSSION BY PROF. EDWARD ORTON, JR.

Mr. Gorsline's apparatus is an exceedingly simple, but exceedingly effective method of showing to burners and others responsible for the burning of claywares, that the loss of water and other volatile substances in clays, is not the simple process that it is popularly supposed to be. It is but a little time since the first printed statements appeared in the clay-trade literature of this country, calling attention to the fact that it requires a red heat to dehydrate a clay, and even now, this statement is met with open incredulity among a large class of so-called practical men. Mr. Gorsline has given them the easiest and simplest manner of proving this point, and while as he says, all the loss may not be water, it at least will convince the most skeptical that *something* is burned out of a clay at red heat, which can not be driven out before.

As a practical tool, for the regular control of burning operations in large plants, I hardly think this process will become important or widely adopted. It is too cumbersome. It would be too much apparatus to move, set up, put in adjustment, and observe, in a plant where from one to five kilns are being started daily. For one kiln at a time, it would be less troublesome.

It would seem that other and easier ways could be reached of getting the same result. For instant it would be just as accurate to put into each kiln, a number of pieces of dry ware, all accurately brought to a standard weight before setting in the klin. If pressed in the same metal die, they would vary but a trifle in any case, and they could be whittled with a knife in a moment, to bring them down so


TRANS. AMERICAN CERAMIC SOCIETY VOL. V.
GORSLINE PLATE II
SHOWING RATE OF LOSS IN WEIGHT.
AS MEASURED BY THE GORSLINE APPARATUS.
Observations made every 30 minutes
TIME INTERVALS. IN HOURS
LOSS OF WEIGHT IN GRAMS
Losses begun after 10 hours firing
Burners changed shift
Burners changed shift
Burners changed shift
Red Heat begun to show
Burners changed shift
Burners changed shift

that they would balance a standard weight on a drug scale or torsion balance.

These pieces, drawn out at intervals of a few hours after redness has been reached, would clearly show whether the clay had reached a uniform weight or not. This method would require nothing but the clay trial pieces, and a small scale or balance. It would not give anything like a progressive curve, from which the rates of loss at different stages could be determined, but it would give the final answer as to when the ware was ready to be carried on up to vitrification.

Regarding the value of the data shown by Mr. Gorsline's curves, I would say that I think that the chief advantage is the education which the man gets who proceeds to do what Mr. Gorsline has done. It pays—tremendously—to prosecute such an investigation. It may help others incidentally, but it is absolutely certain to greatly help the man who plans the work and carries it out. No one can adequately understand or realize these fundamental chemical facts about clay burning, without doing some such work for himself.

When it comes to making exact scientific deductions, or even to making charges of inadequate attention to the firing by the burners, it will require that these curves be most carefully made and studied for a series of burns, before such a course would be safe. For instance, in plate II, where Mr. Gorsline thinks he sees evidence of one of the burners being careless, it seems to me that the evidence is not clear or decisive.

Studying the original curve furnished by Mr. Gorsline, the following losses of weight per shift, and rate in grams per hour are calculated in the following table:

	Shift	Hours	Grams Weight	Rate in Grams per hour
Night Burner	1st	13	22	1.7
Day Burner	2nd	11	100	9.0
Night Burner	3rd	13	66	5.0
Day Burner	4th	11	86	8.0
Night Burner	5th	13	157	12.0

It is not fair to assume that the night burner was less efficient than the day man, because he succeeded in expelling only 1.7 and 5.0 grams per hour for his first two shifts, or that he was more efficient, because he expelled 12.0 grams per hour on his third shift, while the day burner expelled about an equal quantity, 9 and 8 respectively on his first and second shift.

The facts are, that no one could have expelled much water the first shift, without "popping" the pipes. The second shift brought the kiln up to a point when probably the great bulk of the *free moisture* was expelled. By the third shift, the kiln was nearly dry, only a little moisture coming off. This is shown by the fact that the loss was only 5 grams per hour for 13 hours as against 9 grams per hour for 11 hours, on the shift before.

By the begining of the fourth shift, the water again began to come. This time it was the chemical water, which began at the top of the kiln first, and gradually increased as the red heat crept downward. By the begining of the 5th shift, the losses of combined water were becoming general and the rate kept up until the end of the test. Unfortunately, the record could not be carried up to the point where the losses cease, though that point is undoubtly near at hand at clear red heat, and the last observations showed the curve beginning to start upwards rapidly.

If this kiln had been lit at 6 a. m. instead of 6 p. m., it is likely that the work accomplished by the individual burners would have just been reversed. Of course, any wide departure from the proper treatment would show in the curves, and would enable the superintendent to get after the delinquent burner, but conclusions of this sort could only be drawn after a very close study of a number of curves, and with a full understanding of the chemical events taking place in the kiln.

Certainly, the water losses can not become as accurate a guide to the real progress of the burn or the faithfulness and diligence of the burner, as the gain in temperature. In interpreting the water-loss curve, we must make allowances for too many things, while a time-temperature curve, as

plotted from the autographic Chatelier pyrometer is not subject to any such doubt, and may safely be accepted as an evidence of what has actually taken place. However, this instrument is costly, aud available to but few, and hence does relatively little good. There are pyrometers of various other types, which may be used at much less cost.

As the next best thing, to attain an idea of what is going on in a kiln of claywares under fire, I know of no device which is so easily within the reach of everyone, and which will serve to tell so much, as Mr. Gorsline's balance.

MECHANICAL WORK IN THE CERAMIC INDUSTRY.

ADDRESS BY
THE RETIRING PRESIDENT, ERNEST MAYER,
BEAVER FALLS, PA.

A year ago at the Cleveland meeting of this society, you elected me president. At that time, I was unable to be present on account of sickness, and I now take this my first opportunity of thanking you for the honor you conferred upon me.

The American Ceramic Society is now five years old. During this short period its growth and its strength have amazed those who were its founders. Its name is known over this country in circles that we scarcely hoped to reach, and the constant correspondence from abroad, shows that even in those distant points, our work is eagerly sought and its value recognized.

But, this publicity and prominence brings with it obligations which we cannot afford to lightly set aside. We have made a good beginning, and because of that and by means of that, we must keep up our work to the highest possible standard.

I want to make a few suggestions today, in a line where our work seems weak.

The fact impresses me daily that we are sadly behind other industries in our own country. Mind, I don't say other countries, because if we must look to other countries to get aid in the lines I am going to suggest to you, we may rest assured it will never come.

Our society work embraces the whole field of the ceramic industry, and does not, as some people think, refer strictly to the work of the ceramic chemist alone. In meetings held previously, two of my papers have referred strictly to the commercial side of our work, and I want to make a strong plea at this meeting, to see if some enthusi-

asm cannot be raised looking towards bringing the mechanical side of our work into our meetings, to help us out of some of our difficulties.

Outside of a few minor contrivances, there has been practically no evolution in machinery as applied to pottery. Nearly every line of manufacture in our country is now seeking foreign markets, and as we well know, are finding them. We all know that higher wages are paid in the United States than any other nation, yet American manufactured articles are making great inroads into the markets of the world, and the reason is simply the application of machinery and system in our manufacture.

One great difficulty that has always confronted the mechanic, who has brought out new machinery for the more economical manufacture of pottery ware, is this: The requirements of a machine are not clearly understood by him, and when any decided advances are made, it must be done by the really practical potter and the machinist, working hand in hand. Most of the attempts that I have seen made so far, have been where the potter has designed a machine which has failed mechanically, or on the other hand, where a mechanic has designed a machine, and it has not fulfilled the technical requirements of the potter.

I have long been working on several ideas for more economical production of white ware, but so far have yet some important details to be worked out. But in order that my meaning may not be misunderstood, I wish to name a few processes that would make vast differences in the cost of production, if they would be brought under mechanical control.

Most prominently, I would speak of the process known to us as "jollying". I am very positive that every plate, cup, saucer, in fact *everything round*, could be made automatically, where one boy could do more than twice as much as two boys and one man do now. If I am not very much mistaken, such machines will be turning out a very superior class of work to what is now made, inside of two years at the outside.

The process of placing ware in the kilns today, is one of the most expensive methods that could be devised, and

while there is not so much chance here as in the former case for the mechanic, still a very great deal of the heavy, and necessarily expensive work, could be dispensed with.

The process of dipping can be made purely mechanical, and while this statement may sound extravagant in the ears of those who appreciate the value of a good dipper, if any one present is interested enough to take the matter up with me, I will explain exactly what I mean.

Printing is the only process where we see any trace of evolution, and the roller press is here to stay. The identically same machine was used by calico printers in the year 1770; so you see it has taken the potters about 133 years not to invent, but merely to attempt the use of it, and today we can only print in one color. We will only hope it will not take another 133 years to print in more than one color.

The process of making saggers by machinery, is steadily gaining ground in Europe, and wherever they have been used have given great satisfaction.

Brushing all ware in biscuit warehouses ought to be done by machinery, with a proper exhaust fan to take away the dust.

The principal reason that I claim for the introduction of all kinds of machinery in our business, is that on account of the phenomenal increase in the number of potteries in the past few years, and taking into consideration also, all the potteries now building, and just coming into operation, hands must be found to make the ware. Well, we all know the hands will be forthcoming, but how about selling the ware this kind of workman makes? On account of the enormous demand for all classes of earthenware in the past few years, the demand for potters has been far beyond the supply, and the result has been most discouraging to those manufacturers who in the past have prided themselves on the quality of their ware.

The logic of this situation is clear. We need potters, but we need more, the introduction of labor-saving appliances which will place us in position to grow in spite of the shortage of trained help.

Scarcely anything has yet been written by any of our members on the mechanical side. One or two short notes in the earlier volumes is all we have. I hope this little talk will attract the attention of many who have hitherto kept their mechanical papers in the background, thinking that our work was not of this sort. In my opinion, it is particularly needed, both to strengthen the work of the ceramic industry, and to fill out our literature to cover all phases of the ceramic field.

Thanking you once again, for the honor you have conferred upon me, I now invite your attention to the business which the program provides.

ON THE ROLE PLAYED BY IRON IN THE BURNING OF CLAYS.

By Professor Edward Orton, Jr., E. M.
Columbus, O.

The function of a paper before a society like our own, is two-fold. On the one hand, it is to set forth matters of scientific truth, as brought out by investigations in the laboratory or in practice. This is its largest function. On the other hand, it is to stimulate others to study, and to investigate and make known their findings, by placing before them the many fields which remain yet untouched by the representatives of modern science. It is to this end that this paper is directed; no original research has been made to explain its problems, and it is more a query than an affirmation. It simply represents the personal views of the writer gathered by observation in factory and laboratory practice, during the past decade.

The presence of iron in all clays is one of the factors which gives clayworking its fascination, for to this element is due the wonderful capacity for variation which characterize clay products above all others. It is a source of wonder that one element can be capable of producing so vast a variety of effects. The more one studies them, the more marvelous do they become.

When we come to analyze the situation, we soon reach the conclusion that this variety is due to a number of different things, acting in combination. Certainly no single variable could account for the facts. And these influences, so far as known, fall naturally into two classes.

First. Variations in the composition and properties of the clay as regards its iron content.

Second. The treatment of the clay in the burning process.

The natural quality of the clay itself is subject to control or modification to a certain extent, by the clay-worker. The more valuable products, like pottery and encaustic

tiles, justify incurring expense in the preparation of the body, and thus the control of the color, hardness, shrinkage and strength may become very perfect. But for bricks, terra cotta and hollow-wares, the great constructive products, which are used on a tremendous scale, such care in preperation is financially impossible, and we are substantially dependent on what nature has prepared for us.

The treatment of the clay in burning is to a very much larger extent, a matter of choice or skill on the part of the clayworker. He cannot change the character of the clay in most cases, but he can nearly always change the treatment of it in firing, and he can thus often accomplish good results where the clay is itself of poor quality.

VARIATIONS IN COMPOSITION AND PROPERTIES OF CLAYS AS TO IRON CONTENT.

These variations when considered in detail admit of a four-fold sub-division: First, variations in the amount of iron present. Second, variations in the size of grain, or state of distribution in the clay. Third, variations in the mineralogical form in which the iron appears in the clay. Fourth, influence of the minerals accompanying the iron in the clay.

There may perhaps be still other things which go to make up that marvellous and elusive thing which we recognize as the character or individuality of the clay. But, so far as the influence of the iron is concerned, the four headings above undoubtedly account for most of the phenomena with which we come in contact.

Variations in Amount of Iron Present.

Variations in amount are the first recourse to which writers have turned in endeavoring to explain the color phenomena of clay burning. It only takes a superficial acquaintance with the subject, however, to find that this factor is much overrated in importance, compared with the chemical condition of the iron before and after burning, and its physical distribution in the clay.

In general, we may say that no clay is free from iron. The amount may be truly insignificant, as in the purest white kaolins which sometimes only show a few hundredths of a percent, or it may rise to such proportions that the clay ceases to be regarded as such and is valued as an ore of iron. The clay-iron-stone of England and the paint called ochre are cases in point. But the usual proportions are far removed from either of these extremes, and are sufficient to destroy the white color of kaolin on the one hand, and the fine red color of the iron oxide on the other.

Seger has covered this field, as he has most others in the ceramic world, and has shown that clays should be divided into three well marked classes, as regards their iron contents, viz, the white-burning, buff-burning and the red-burning.

White Burning Clays. Without going fully into the conditions which have brought these things about, we may say that the white-burning clays represent those rare conditions, where a rock low in iron has weathered and broken down into a clay, without the intrusion of iron-bearing water or sediments from neighboring rocks. Such a combination of events does sometimes happen, and hence we sometimes find clays which burn white. But since they can only grow by the weathering of igneous rocks, they come almost wholly from the mountain districts, where granites and similar feldspathic rocks occur.

Since transportation of clays in streams is almost certain to result in the blending of iron-bearing sediments with them, it follows that white burning clays in the vast number of cases are primary, viz, found on the site where they have first been formed by weathering. But, in some few instances, as in the Florida kaolins, white clays have actually been transported over long distances and redeposited as secondary beds, without collecting enough iron to throw them out of the white burning class.

The rarity of the conditions producing white-burning clays naturally makes them a very small item compared with the vast bulk of other clays. They probably comprise only a few hundredths of a per cent of the total clay of the

world. Their economic importance is in proportion to their varity, however, and they have thus become a commodity of much value, ranging in price up to $10.00 or $15.00 a ton for the choicest grades.

White burning clays carry from a few hundredths of a per cent of iron to considerably over one per cent. The more ferrugineous contain much more iron than the purer grades of the buff-burning clays, whence it is evident that quantity alone is not a sufficient explanation of the color.

Red burning clays. It has been shown that it is a rare and unusal event when a primary rock breaks down into a clay, or a secondary clay is built up from a floating sediments, without becoming stained with enough iron to bring it out of the white-burning class. In fact, the vast bulk of igneous rocks are ferrugineous, and break down into red burning clays when they weather.

But the remarkable thing in this connection is the regularity of the iron contents of red burning clays. Of the known clay strata, the shales, the glacial clays, and the alluvial clays comprise the enormously preponderating quantity; probably 99 per cent of the whole. And in this great mass, the ferric oxide remains ordinarily between 4 and 7 per cent; occasionally it rises to higher figures. Rarely, under the influence of local causes, it becomes iron ore. But shales from every state in the union, and from every country of the world, the loess of the Mongolian steppes, the glacial clays from Arctic and Antarctic regions, the alluvial clays of Nile, the Ganges and the Amazon, are colored alike with this uniform and ever present constituent.

The explanation seems to rest on the fact that iron composes a small per cent of the known elements of the earth's crust, and that as fast as it is unlocked from its combination in the plutonic rocks which make the shell of the earth, it tends to undergo a concentration in the shale-clay strata. Its fineness and lightness in the form of ferric-hydroxide, which it takes on oxidation, prevents its retention among sands to any large extent. Consequently, sandstones contain less than their share of the total iron. Likewise limestones accumulating from organic sources in

clear water, usually are low in iron. But the shale clays, being composed of the finest sediments, the sludge which travels farthest and settles last, naturally contains the bulk of the ferric hydroxide, which is equally fine and equally difficult to settle.

The origin of the shale-clays, then, is thought to explain their uniformity of color, and the intimacy with which their iron contents have been blended into the other ingredients of the clay. And from the weathering and erosion of the shales, the vast bulk of the glacial and alluvial clays have come, so that we may predict with considerable confidence that red-burning clays, whatever their age or present location, contains between 4 and 7 per cent of ferric oxide.

The red color of red-burning clays is not in proportion to the iron contents however. That is, many clay containing 4 or 5 per cent of ferric oxide burns as fine a red color as others containing 7 or 8. Here again, the distribution seems more important than the amount. While we do not find clays burning red without some considerable iron in their make up, we do not find all clays burning red which contain this amount of iron.

Buff-Burning Clays. This group is less clearly understood than either of the others. In amount of iron content, it varies from 0.50 per cent to 4 or 5 per cent, ordinarily being about 1.5. It overlaps both the preceding groups. White-burning clays burn white because they contains so little iron. Red-burning clays burn red because they contain so much iron. But buff-burning clays do not burn buff because of the exact amount of iron they contain. Often they might burn either red or white, so far as the iron content is concerned. Obviously, quantity alone is a less perfect explanation for this group than for either of the others.

A discussion of their origin leads too far at this present point. Suffice it to say that there are certainly two sorts of buff burning clays, with different histories, and whose color proceeds from different causes. In both, the color is thought to be due to the chemical influence of other elements on the iron oxide.

Variations in the Size of Grain or State of Distribution of the Iron.

The powerful influence of the physical factor is well realized by all who have tried to stain clays artificially to some particular tint. Iron oxide in masses of appreciable size becomes red on calcination at a low temperature. changing gradually to a bluish or violet black as the temperature increases. When added to a clay, it appears as dark colored grains in the matrix of the clay, which is itself changed little or none by this addition. Even when we grind ferric oxide to an impalable powder, and distribute it into a clay most perfectly, it merely causes a darkening or browning of the color, nothing like the buff or red colorations of natural clays.

The failure of these simple synthetic attempts to produce iron colors in clays leads us to a study of the condition of the iron in natural clays. We find two states easily distinguished. First, the precipitated or colloid form, whose fineness exceeds all methods of measurement and which we may fairly assume to be almost molecular. Second, the concretionary or granular form, in which its grains or crystalls are of appreciable size.

The precipitated form is necessary to the development of either a buff or red color. We may easily verify this by precipitating artificially some ferric hydroxide into a fluid slip of clay. It is possible to produce colors as uniform and as free from specks as natural clays, but it is not possible to get some of the tints of natural clays or to get as much color for the amount of iron used as is developed by Nature's methods. As shown in discussing the origin of the shale clays, the iron has been associated with the clay minerals from the very time of formation. The primary clay was usually deeply stained by the solution of iron which formed with it. The soft gelatinous hydroxide has enveloped its grains form their very origin, and left each tiny particle covered by its coloring film. Every succeeding process tends to increase this uniformity, grinding, transportation, erosion, deposition in swamps, all tend to increase the iron and to reduce its blend to perfection.

Thus in the ordinary red-burning shale-clay, for instance, it becomes impossible to distinguish the hydroxide, from the clay minerals. The sand, and the mica, and the concretionary minerals of all sorts, can readily be separated by screens or by sedimentation processes, but the pulp which remains inseparable to the end, contains the bulk of the iron, and is red-burning, while the coarser matters which have been separated out, may burn light or dark or speckled, but hardly red. In fact the question has been raised as to whether iron is not chemically combined as a part of the clay substance. No proof of this view has ever been attempted, and all the evidence obtainable points the other way. For instance, dilute hydrochloric acid will take out nearly all the iron from a red burning clay, leaving a residue in which the clay substance has not been sensibly attacked and which burns light colored or white. If the iron were a part of the kaolinite, its withdrawal would leave the molecule decomposed, and the alumina also would then dissolve, as its bond with silica would be broken. But such is not the case. Hence, while we can readily see how such a view might be suggested by the extreme intimacy of the mixture, and the uniformity of the color produced by it, still it cannot be defended as a scientific fact.

But here, again, we reach a point where qualification becomes necessary. The occurrence of the iron in infinitely finely divided form is necessary to the development of either the buff or the red color. But, the presence of iron in this form, even in connection with the quantity, does not in itself necessarily cause the red or the buff color. The addition if even a little red clay slip to a buff slip will produce a body which burns salmon or flesh color or terra cotta, while other buff bodies may easily be found containing twice as much iron as the experimental mixture contains. Thus it appears that each clay is buff or red as the case may be, from some cause other than the amount or fineness of distribution of the iron.

The concretionary forms of iron in clays include all particles which were not in the jelly-like or colloid state when they entered the clay or which have been drawn to-

gether into masses by the obscure forces which we call crystallization or concretionary growth. It does not matter what the force has been which collects iron into granules or what the history of the granules of iron in a clay has been. The mere fact that it *is* in granules, is sufficient to determine its effect.

In general, we may say that its coloring power is almost nil. That is, its power to color the matrix or real pulp of the clay. It will produce colors in the broad sense, for we add iron to buff-bricks to make them into grays or speckles. But the change in color is due to alternations of black and buff, and not to a blending of colors, or, the formation of a new color. This segregation of the iron of a clay into granules, is the cause of much trouble to the clayworker. With this trouble comes some benefit. The trouble consists in speckled, spotted, pimpled, rough and unsightly ware. The benefit comes from making a virtue of necessity and creating a market for such speckled and mottled color as concretionary iron will produce. The face-brick industry has shown a wonderful example of this in the last 20 years, and now millions of mottled and speckled and gray bricks are produced with good artistic results. Yet, the real reason for their appearance was the inability of the clayworker to always avoid such manifestations in his wares.

We find that even concretionary grains of large size do contribute some little to the color of the body-matrix of a clay, when the firing temperature becomes sufficient. The iron assumes a silicate combination, and permeates the surrounding matrix, forming a ring or shell of yellow or red or black color, whose size depends on the fluidity of the iron silicate and the duration of high temperature. But, practically speaking, concretionary iron is not responible for the colors of clay.

The Mineral Forms in which Iron occurs in Clays.

The ferrugineous minerals which are most commonly found in clays are:

(a) Ferric Oxide, anhydrous, or in various stages of hydration.

(b) Ferrous Carbonate.

(c) Ferric Sulphide or Pyrites.

(d) Ferrous Silicate minerals, like Biotite, Hornblened and many others.

(e) Ferric sand minerals, like Magnetite, Menaccanite, Chromite, etc.

The two latter divisions do not contribute any large quantity of iron to ordinary clays, probably not over one per cent. and generally much less than that. They are represented by small amounts in all clays, but they affect the clay chiefly by forming minute black slag spots or black specks in the ware, according as the heat has fused them, or left them unslagged. It is from the three first minerals, therefore, that we derive the principal beneficial or detrimental effects of iron.

These three minerals are sometimes all found in the same clay, and it cannot be said that any one is confined to any particular kind of clay. Still, some sort of classification by location is possible.

In the following discussion, consideration will only be given to the secondary clays, which compose the bulk of our clay wealth. The primary clays, although they include nearly all of the white-burning kaolins and china clays, form such a minute part of the vast masses of clays in general, that they need not be considered in this discussion, and the red burning primary clays are of small economic importance.

Among the secondary clays those of recent geological age, like the alluvial clays which cover the present river valleys and bottoms, almost invariably contain their iron in the form of ferric oxide or hydroxide. Ferric sulphide and ferrous carbonate are not unknown in such clays, but they are quite unusual.

In the next group belong the glacial clays, which were deposited like a blanket over the northern half of this continent, by the great ice cap or continental glacier which invaded us from the north in recent geological times.

The rough hilly country was covered with the ice sheet, the tops of the elevations ground off, the valleys filled in

with the debris, and over all, as the ice retreated to the north again, the great sheet of clay was left behind to form the soil. In view of their origin, we cannot expect glacial clay to be uniform in any respect, either in size of the mineral particles, or the kind of minerals represented, or their chemical condition. And the facts justify this view, for no sedimentary beds vary so widely in character as those laid down by the ice.

In the glacial clays, ferric hydroxide predominates; especially is this true on the surface. But, where deep cuts are made into glacial clays, we not infrequently find them taking the form of blue, tough, adhesive hard-pan, in which the iron is ferrous carbonate. It may be fairly said that time has elapsed since the glacial period, to convert the iron salts of the deeper strata into ferrous forms. But, in many cases, the other conditions did not favor the change. Hence, while ferrous beds are common, at times almost reaching the surface, they are more often found deep under cover.

Clays of ancient origin, such as the fire clays and shales, contain their iron most commonly as the ferrous carbonate or the sulphide. The reason for this is clear. Nearly all such strata contain, or have contained, more or less carbonaceous and sulphurous matter, and during the enormous periods of time since their deposition, have been subjected to bacterial action, heat, and pressure. The reduction of the iron, and its combination with the CO_2 and S thus produced is a natural event under the circumstances.

All fossil clays are not necessarily ferrous, however. We have among the Carboniferous and also in the Permian, many conspicuous examples to the contrary. Red shales and red clays, which contain their iron largely or wholly in the ferric condition, are not uncommon, geologically speaking.

The sulphide of iron is a "free lance," so to speak. It is common, both in old and young rocks. It is easily formed and as easily decomposed again. Oxydation, even at common temperatures, rapidly converts it into the sulphate, while similarly, the sulphate is easily reduced by decaying

organic matter to the sulphide once more. Hence, its presence means either recent reduction, or that no opportunity for oxydation has recently occurred. It is called a ferric mineral, but in the conditions of its formation, it accompanies the ferrous group, and when it decomposes it invariably yields the ferrous oxide first. So, while it is a ferric salt, it ranks with the carbonate in its tendencies in the clay.

But while shales and fire clays characteristically contain their iron in carbonate or sulphide form, it is also true that the exterior of most shale-clay and fire-clay beds have been more or less deeply oxydized by weathering processes. Whatever is easily accessible to air, percolating waters, and the mechanical action of heat and frost, is likely to become ferric, while rocks below drainage level, or covered deeply by other strata, are apt to remain ferrous.

The effect of these various minerals on the color of the clays is not in itself strongly marked or characteristic. That is, as good a red color may be developed from a clay containing its iron as ferrous carbonate as from ferric hydroxide. But the mineral form is of importance nevertheless from two causes. First, the tendency which some minerals show to crystallize or gather in concretionary masses. Second, the great differences in treatment which the different minerals require in burning.

As to the first tendency, it may be said that all forms of iron crystallize or gather in concretions. It is simply a question of degree. Ferric oxide and hydroxide is least prone to do so. In fact, where ferric concretions are found in clays, they generally represent reoxydized concretions of the carbonate, rather than accumulations originally ferric. Consequently a clay containing ferric oxide exclusively is apt to be of an even color on burning.

Ferrous Carbonate, on the other hand, seems to exist in disseminated form or in concretions with equal ease. In fact, both forms generally exist side by side. A red burning matrix will be full of black specks, produced by the lumps of the same mineral which produces the red color. Thus in dealing with a clay in which ferrous carbonate is known, there is every reason to expect that finer grinding

and greater care in preparation will be necessary, unless the ware is one in which the presence of specks is not prejudicial.

Ferric Sulphide, on the other hand, is invariably found in granular form. These granules may be large or small, but they are never small enough to give a red color. They invariably show up in the burned products as black specks or slagged spots, and hence in most wares are an unqualified nuisance. An interesting case of some kaolinitic clays from the Phillipines recently showed pyrites in every sized grain from 8 mesh down to 200 mesh, the latter requiring a glass to see the grains clearly. The pulp passing the 200 mesh seive burned gray, evidently from the little grains of pyrites which the seive had not been able to retain, as the clay itself burned pure white, in occasional places where no sulphide of iron was present.

The effect which these three minerals exert on the color of the clay through differences in burning, will not be discussed at this time except to say that ferric hydroxide is the easiest to burn successfully, the carbonate next and the sulphide most difficult.

The Effect of Other Minerals on the Iron Color.

The considerations involved in the quantity, the distribution and the mineral form of the iron, have been shown to be accountable for much of the variety of color in the common or secondary clays. But, it has been shown that some of the fundamental phenomena are still unsatisfied by these causes. The development of the buff or yellow color is the chief of these. We have seen that we cannot produce it synthetically with anything like the ease with which the red is produced. In fact, the writer has never succeeded in making a good buff from a white, by addition of iron. The use of iron in white clays has given a pink series, but no yellow. It seems that the latter is due to another factor, viz, the masking or suppression of the iron color by other elements in the clay.

Seger has classified buff burning clays into two groups. First, those high in iron, which would burn red normally,

but which contain so much lime as to destroy the red iron compound and form a yellow lime-iron compound in its place. Second, those clays low in iron, which would burn to a pale red at best, but which contain enough alumina to form an alumina-iron compound of a yellow color. Seger thinks the destruction of the iron-red color in these two cases is accomplished by similar means, and that lime differs from alumina in being the stronger and more active base, and thus being able to overcome the greater quantity of iron.

All agree on the former proposition. The effect of lime on the iron-red color in clays is undeniable. The reaction is in daily use as a means of controlling iron colors, all over the world. But to this second proposition, the writer can not give unconditional assent. The evidence Seger marshalls is strong and undoubtedly points to his conclusion. But if it were true, then synthetic mixtures should easily give the buff color, which has not been the writers experience. We certainly find an amazing uniformity in the color of buff-burning clays, while their iron-alumina ratios fluctuate very greatly. Some fire clays contain 40 per cent. of alumina and 0.5 per cent. of iron, and burn a good buff. Others contain 15 or 20 per cent. of alumina and 2.5 to 4 per cent. of iron and burn to almost exactly the same tint. Other clays of different geological history, containing about the same alumina and iron, burn a fine red color.

It seems as if some other influence than either the amount of iron, its distribution, or its masking by other oxides must be brought in to explain the buff-burning fire-clays. The significant fact is that fire-clays occur so very generally in connection with coal veins or other accumulations of organic matter. Wherever carbon has been accumulated in bygone ages, there we find buff-burning clays immediately underneath. Often the coal has been washed away while still soft, and disseminated, but the fact that it had been, is proven by the fire-clay vein which is left behind. In fact, the fire-clay more uniformly marks the coal swamp, than the coal itself.

This connection, so constantly shown all over the world, in strata of all ages where coal has grown, certainly seems

significant. Whether the swamp growth has formed certain organic salts of iron which influence the color, or whether it is by the removal of other substances than iron, or whether it is by stimulating the growth of concretionary forms of iron and thus preventing the distribution of the color, can not be stated.

On the other hand, in support of Segers theory that alumina masks the iron and produces the buff color, the fire-clay beds of New Jersey, the flint clays of Missouri, and many of the Cretaceous fire-clays of Germany, may be cited. They do not occur in connection with coal veins, nor is it reasonable to suppose that they are composed by the erosion of other carboniferous fire-clay beds, and deposited without sensible blending or intermixture with red burning sediments. In fact, their occurrence, alternating with beds of sand, gravel and red-clays, gives the lie to any such assumption.

The most we can say at present on the cause of the buff color of the fire-clay groups, is that they generally contain less iron than is needed to produce a red color; that this iron is distributed in the most perfect manner possible. Segers alumina theory does not seem to wholly fit the case, and the geological history of fire-clays and their production by swamp growth and carbon accumulation is after all not a cause. There must be a difference in compound, or amount, or distribution, or their geological history could hardly avail to produce a difference. The settlement of this most interesting point is one which invites the attention of the ceramic investigator.

The effect of the accompanying minerals on the color generated by iron is not confined, however, to the reaction known as "masking," which have just been discussed. There is another class which is a prolific cause of trouble in getting the usual iron colors, but whichhas no off-setting advantages in the way of producing desirable new colors. I refer to the presence of reducing agents, like carbon, hydrocarbons or organic matter, or sulphur compounds. These substances all burn out from a clay readily under favorable conditions. They do not, of themselves, add to or subtract

from the colors wrought by iron. But, by virtue of their superior affinity for oxygen, they prevent or delay the iron from playing its customary role, and hence cause untold trouble in the burning.

The ancient or fossil clays are the ones most affected by these troubles. Recent clays, like the river and glacial beds usually contain some trifling amount of organic material, but it is still woody in nature and hence easily combustible. It burns out early in the process of firing, and leaves the ware spongy and easily accessible to the kiln gases. Hence, it does no harm except when it becomes excessive in amount and thus makes too much heat.

But shales and fire clays have been generally altered by heat, pressure and bacterial action, and their organic matter has been turned into graphite, or coaly matter, or bituminous or asphaltic cements which penetrate the clays most intimately. Also sulphur, which is seldom found in quantity in the surface clays or the young clays, is nearly always common in the old or fossil clays.

The most difficult clays to burn which the clay worker ever meets are the bituminous and pyritiferous shales and fire clays. It is safe to say that the losses incurred in clay burning center largely around the troubles which these minerals aggravate, if not actually cause.

THE TREATMENT OF CLAYS IN BURNING.

We have now seen that the occurrence of iron in clays is so varied as in itself to suggest a cause for the complexity of results which practice demonstrates. And, there is no question that much of the richness and variety in clay products does come from just this fact. But, the treatment which the clays receive in burning is a matter of fully equal concern to the clayworker, because the strength, durability, beauty and color, texture, in fact the whole quality of his product, is affected or controlled thereby. It is a matter of of even greater concern, because it is largely under the control of the clayworker himself, and is thus a direct key to commercial success, while he cannot exercise a similar con-

trol over the composition of the clay itself, except for fine wares, out side of the range of this discussion.

It is strange perhaps that iron should play a part of such exceeding prominence in clay burning, but such is the case. It not only has the power to color the products though a multitude of varying shades and tints, some beautiful, others unsightly, but it also has the power to cause the ware to swell up, to become deformed, to become pimply and rough on the surface, to become spongy and weak, in fact perfectly worthless. In order to guard against these troubles, it is necessary to understand their causes, and this involves an understanding of the whole theory of the burning and vitrification of claywares.

Clay burning is chemically a most complex process; it consists of a series of reactions overlapping one on the other in such a manner that one can never trace the exact beginning or end of any single one. In general, we may divide it up into three principal kinds of reactions, and these divide the burning process roughly into three corresponding stages.

First. The dissociation of materials which are unstable at high temperatures, and the expulsion of the volatile portions from the mass of the clay.

Second. The transformation of the non-volatile elements from less stable into more stable forms by absorption of gases from without the mass.

Third. The amalgamation at an elevated temperature of the various residual inorganic substances left by the two preceding reactions, into a more or less perfect silicate compound.

Of the reactions which make up these three classes, there are some which are necessary to the successful execution of the burn; others whose influence is seemingly not necessary or important; others which cannot be construed other than injurious to the product, and to be avoided as far as the case permits

The division of the burning into these three periods is of course arbitrary, since the reactions overlap, and at no

time can it be said that one process only is going on. However, it affords some measure of convenience and intelligibility to a process which is otherwise complicated, and may be justified on that account.

THE DEHYDRATION PERIOD.

The reactions of this period are only the preliminaries by which the minerals of the clay are brought into condition for the final combinations which culminate in the finished ware.

The reactions are chiefly

(a) Expulsion of hygroscopic water from the clay.

(b) Combustion of carbonaceous matter.

(c) Distillation of S from pyrites, leaving FeS.

(d) Dehydration of Kaolinite, Ferric hydroxide and other hydrous minerals.

(e) Decarbonization of carbonate minerals.

Taking each in turn we may describe them as follows:-

Expulsion of Hygroscopic Water.

This is not a chemical process, but its power to interrupt or defeat chemical processes is so great that it seems proper to give it a place.

Water is vaporized at 100° C., and theoretically cannot exist longer in the clay in the free condition after that temperature is reached. But practically, the pores of the clayware retain it obstinately until temperatures considerably in excess of 100 degrees are reached in the general atmosphere of the kiln. The kiln is hardly to be considered dry until the atmosphere of its coldest part has reached 150° C.

The power for ill which this process exerts lies in the delaying of other processes, by which the time devoted to their execution is shortened. No oxydation or dehydration can occur while the mechanical water is present, and further, no senible headway can be made in heating the ware up until the water is gone. And since the heat required to evaporate water is 537 units, and since the specific heat of water below boiling is five times as great as that of clay, and since the specific heat of steam is twice as great as that

of air, it is plainly to be seen that where two pieces of ware, one wet and one dry, are both subjected to the same kiln gases, that the dry one will attain a high temperature rapidly, while the other will gain no heat for a long time. Hence, when the temperature reaches the point where some other chemical reaction is due to begin, one piece is prepared and the other is not prepared for it.

The practical importance of this point is exceedingly great. The simple remedy is to never set ware in the kiln unless it is dry, or if for any reason ware must be set which the burner knows is too wet, or if some wet ware has been interspersed through the dry ware, then he must convert his kiln into a dryer, and fire it so slowly and so long before venturing to raise fires, that all original inequalities of water will be removed.

Many very intelligent burners and managers believe that the swelling or bloating of clay wares is due to the water escaping at high temperatures. In fact, the writer himself advocated this theory in 1892. But further study has shown that wet ware is only the indirect cause of the trouble. The actual swelling takes place by another chemical reaction. But if the water had been properly expelled first, the second reaction would not have happened.

The picture (Fig. 1) shows an example of this difficulty which is common in most paving brick plants. Two brick, stuck fast to each other in burning, one swelled, and the other square and true. The swelled brick was set damp, while the square one was dry. Yet both must have had the same conditions of time, temperature and chemical composition of kiln gases.

Combustion of Carbonaceous Matter.

This reaction is one which may cover a very great heat range depending on the kind of carbonaceous matter present. There are several principal forms:

1st. Vegetable tissues, fresh or but little altered, such as rootlets, grasses, leaves, bits of wood, or wood pulp, turf, or peat-like manner.

2nd. Vegetable or animal matter, greatly altered by

FIG. 1. SEE PAGE 394.

FIG. 2. SEE PAGE 396.

partial decay, or partial distillation under heat and pressure. Here belong the lignitic matters found often in ball clays, and the bituminous and asphaltic matters found in the shales of the Carboniferous and earlier periods.

3rd. Inorganic carbon minerals, in which any organic structure they may have had has been blotted out by the distillation and heat treatment they have undergone. In this class belong the graphite found in the shales and slates of the Appalachian mountain district.

Besides these substances which naturally occur, there are others, added for various technical ends, which fall more or less directly into these classes: for instance sawdust, tan-bark, and straw in the first, and anthracite coal-dust and coke-dust in the third division.

The first class, or woody tissues, burn out rapidly and at low temperatures. Wood catches fire even as low as 250 or 300 degrees, and if the amount is small, this class of substance rarely does any harm. If it is large, as in the making of fire proofing or terra cotta lumber, where some times as much as 40 per cent of sawdust is used, it is capable of ruining the ware, by bringing the heat up too fast.

The second class, or bituminous matter, is the most common and most dangerous form in which combustible matter may be found in clays. Its danger lies in the fact that it is a partly volatile substance, solid at ordinary temperatures but which readily breaks up into a rich combustible gas, and a residue of carbon or coke, by the raising of the temperature. Three to six percent of such carbon is often found in a shale clay, and this means, in a kiln of 50000 bricks, six to twelve tons of fuel, or a third enough to burn the kiln, if its heat could only be utilized.

The volatile portion of these bituminous matters naturally burns very rapidly and with intense heat, when the temperature reaches the point where it is expelled. In fact its combustion raises the temperature of the kiln so rapidly when it once catches fire, that it urges on the completion of the distillation far faster than the normal heat of the kiln would do, and does not cease until every bit of volatile matter has been expelled and combusted. But by this time,

the temperature of the kiln will likely have passed all safe limits and the ware have become swelled, melted and stuck fast together.

Fig. 2, represents a collection of bricks from Lorain, O., where a black shale containing 6 per cent. of bituminous matter was being worked. Spalls of this shale thrown into a hot kiln would blaze nicely for several minutes. The company was making brick by the dry process. After spoiling many kilns, they were finally told to shut off the air supply, when the gases took fire, and let the conflagration within go on slowly until it finally burnt itself out. The leakage of air through the walls of the kiln and through the daubing of the fire holes and wickets was sufficient to combust more gas than was advantageous. Unrestricted air supply simply melted the bricks. Five days of this slow distillation was sufficient to drive out all but the solid coke residue. The kiln was then opened, air admitted, the last of the carbon burnt out and the work proceeded.

The brick on the top shows the brick properly burnt. Those below show the results of unrestricted air admission and consequent melting. The danger from this form of carbon is not wholly confined to the too rapid combustion of the gases. The residue of the carbon left in the clay when the gas goes out, is still a troublesome problem. It is so fine, so evenly distributed, and so intimately blended with the clay, that it is much more difficult to burn out than ordinary graphite or coke or anthracite slack.

The cut (fig. 3) shows a collection of draw-trials made from a moderately bituminous shale from Cannelton, Indiana. The shale had passed a 16 mesh screen and was worked by the plastic process. Beginning on the left, the first trial was drawn after 20 hours firing, 10 of which were above red heat. No. 2 was drawn at 24 hours. The color of the uncombusted carbon in the core was paler, but not reduced in area. No. 3 was drawn after 30 hours. The carbon had burned out somewhat, as was shown by the reduced size of the black spot. No. 4 was drawn at 37 hours, showing the carbon practically gone. The kiln was then fired till 44 hours, and No. 5 was drawn when cold. The

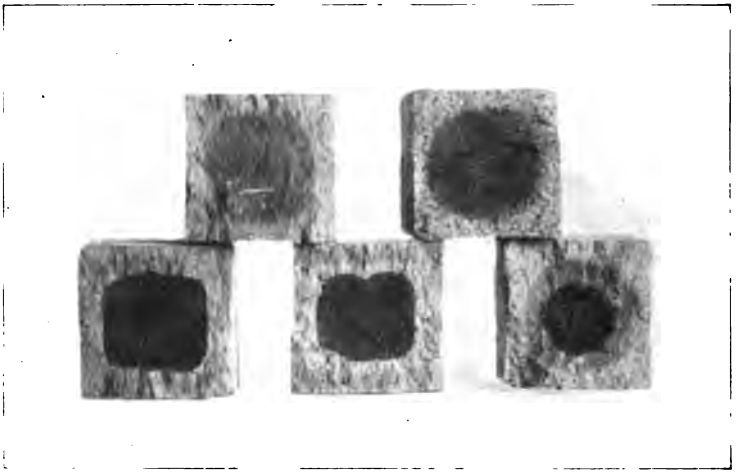


FIG. 3. SEE PAGE 396.

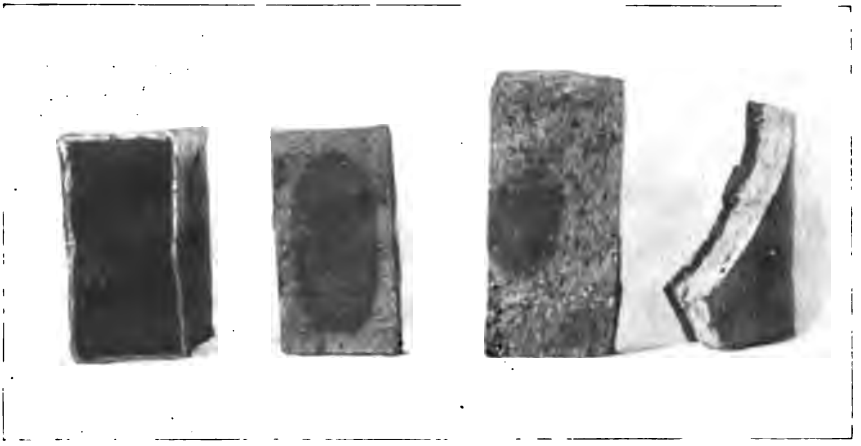


FIG. 4. SEE PAGE 410.

central core is of a brighter, more orange-red color than the exterior. This shows first in No. 3, encircling the small black core. No. 4 shows it also, but No. 5 shows it most strongly developed. This difference in color is due to the fact that the iron oxide has been reduced by the carbon, and although reoxidized perfectly, it never takes the same color again. It is, in fact, the same reaction discussed by Bleininger under Notes on Flashing*

Considering the small size of these brickettes, and the special regulation of the test kiln atmosphere for oxidizing the carbon, the above series is a very instructive example of the slowness aud difficulty with which this is accomplished. Where a whole kiln of such wares is to be treated at once, the difficulty and necessity for time are correspondingly increased.

The third class, or non volatile carbon, is not often a source of danger or inconvenience to the ware, especially in relatively coarse or granular forms. It burns only by slow oxidation, or glowing, and without flame. It does not burn out until a good red heat is reached, and requires that time be given for this to take place. Otherwise, it is seldom a cause of trouble. In fact, it is often purposely added to a clay, to make a better distribution of heat in the kiln. The brickmakers of the Hudson River district are especially addicted to the custom.

As will be seen later on, the carbon compounds are not *in themselves* particularly troublesome or hurtful to a clay, for they always disappear in the burning if given any chance, and are not often in sufficiently large quantity to exercise any great effect on the working qualities of the clay while raw. But they are a most fruitful cause of trouble indirectly, by their influence on the iron compounds, as will be shown later.

Distillation of Ferric Sulphide. FeS_2 is a mineral which is perhaps responsible for as much of the trouble incident to clay burning as any other single one. Its heat reaction is peculiar in the fact that the sulphur is not driven out at one

*Transactions Am. Ceramic Society, Vol. II page 74.

time, but in two stages. The first decomposition is represented by the formula

$$FeS_2 + heat = FeS + S.$$

This reaction can easily be verified by heating a little pyrites in a glass tube, to the point of redness or just below. The first molecule of sulphur comes off, forming yellow oily drops on the side of the glass; the black residue remaining is found to contain one-half of the original sulphur.

The second molecule of sulphur is obstinately retained by the iron, and can only be removed by roasting with free oxygen in abundance. The point at which this is completed will depend on the porosity of the clay, the rapidity with which the heat advances, and the purity of the hot air which has access to the clay. If it is largely diluted with combustion gases, the oxidizing power is, of course, much cut down.

It is safe to say that in ordinary cases, the first molecule of sulphur from FeS_2 is expelled and burns to SO_2 or SO_3 at temperatures of 400 to 600°C. It is equally safe to say that that the last molecule is not expelled at all, unless due opportunity is given. Under favorable circumstances, it undoubtedly is roasted out by 900°C, and perhaps earlier. Where this does not occur, the FeS fuses, forming a black slag.

Dehydration of the Hydrous Minerals. The chief of these is the kaolinite, which loses its water at a low red heat, variously estimated at 550° to 650°C. It is now the opinion of the writer that the expulsion of combined water from clay, or from any other hydrous mineral, cannot be the cause, in itself, of any of the troubles incident to clay burning. An opposite view was entertained and published in 1892*, in which the swelling and blackening of red and buff burning clays was ascribed to this cause. Langenbeck in a private communication, was the first to call attention to the impossibility of this explanation being true. There is no doubt that clays have, at the completion of the dehydration reaction, a structure more porous than at any time, before or

*Ohio Geological Survey Report. Vol. VII, page 51.

after, and the swelling of clays is now explained in another way.

Besides kaolinite, water of combination is found in various other minerals, gypsum, talc, various zeolites, and the hydroxide of iron. and manganese. Undoubtedly they all lose their water quietly and easily, as kaolinite does, and at as low a temperature.

The Decarbonization of the Carbonates. Of these minerals there are four commonly found in clays, and several may be found together in one clay. These are: Carbonate of calcium, carbonate of iron, carbonate of magnesia and dolomite.

The carbonate of lime, so common both in and out of the clay strata, is a constant factor in clayworking operations. Only two classes of clay are presumptively free from it, viz., fire-clay of the coal measures and alluvial clays of recent origin. Shales may or may not be free, and bog clays and glacial clays are pretty sure to contain it.

The heat reactions of calcium carbonate have been carefully studied by the lime and cement chemists, and from them we know more of its behavior than from the researches of clayworkers. It decarbonizes at 600 to 900°C, the time depending on the composition of the kiln gases. The more steam is present, the more readily is the gas given off, though why that is so, is a matter of conjecture. The decarbonization is certainly delayed if CO_2 is present in large quantity in the gases, and CaO once formed will actually revert to $CaCO_3$, if the CO_2 gets too high a constituent in the surrounding gases, and the temperature is not too high. Above 900°C, the reabsorption of CO_2 is not possible, until the temperature drops again.

Carbonate of iron, or siderite ($FeCO_3$) is a very abundant mineral in clays. As already indicated, it is found chiefly in the shale, and generally not in clays of recent deposition. The former greatly outweigh the latter in quantity, but the latter are more widely distributed and accessible as well as more plastic, and hence respond more readily to the clayworkers needs. It is natural, therefore, that

the clayworkers should have avoided the former as they have done.

But in comparatively recent times, shale clays have sprung into great favor, chiefly for their undoubted fitness for making vitrified wares, like sewer pipe, paving brick and hard building brick. And in using them, clayworkers have come into frequent contact with a series of phenomena for which their older practice with superficial clays had not prepared them.

The behavior of $FeCO_3$ on heating has been made the subject of one incomplete series of tests in my laboratory. We found that at 425°C, the CO_2 had been fully expelled. How much lower than this point the reaction began, we have not yet determined, as the first brickettes drawn at 425 showed the CO_2 already expelled. The test was made on a kidney iron ore which contained about 35 per cent. of iron as carbonate and 6 or 8 per cent. as ferric oxide. The ground ore was plastic enough to endure being molded into brickettes without the use of any clay as a binding agent.

These results were a surprise, as the idea had been accepted previously that $FeCO_3$ retained its CO_2 vigorously till 800 or 900 degrees.

The decomposition of magnesium carbonate and of dolomite takes place at about 400°C and 600°C. The latter figure is not accurately known, but it is known to be between the point where calcite and magnesite break down, and much closer to calcite than to magnesite.

The small quantities and infrequent occurrence of magnesite in common clays makes these figures of much less moment to us than is the case with the carbonates of lime or iron.

The decomposition of the carbonate minerals is a very important and significant thing in a study of the vitrification process in clays, on account of the reactions which begin between the basic oxides thus formed and the silicates of the clay. But their interest in this present connection is chiefly because of the carbonic acid gas which must be expelled before the oxydation of the iron can enjoy a free scope. The genuine obstacle which the carbonate clays

offer to satisfactory oxidation, is apparent to all who deal with them. Limey clays especially resist it, because their pore system is clogged with the escaping CO_2 until solate; 900° or thereabouts.

The heat behavior of ferrous carbonate differs from that of $CaCO_3$ or $MgCO_3$, in the fact that FeO, which remains when the CO_2 is set free, is not a stable compound. In fact, it is one of the most unstable; it is difficult for the chemist to produce and keep FeO, free from oxidation. Consequently, the breaking down of the $FeCO_3$ is only the signal for a second reaction, viz., the oxidation of the resultant ferrous oxide to the more stable ferric form. This reaction will be fully discussed in the succeeding section.

In the foregoing, has been set forth in some detail the general behavior of the minerals during the heating from atmospheric temperatures up to 700°C. No single clay is likely to contain all of the various minerals here noted. But on the passage of any single clay to this temperature, it is believed that the order of the changes and their results would be about as described.

The Physical Structure of Dehydrated Clay. It is important for us to realize the physical condition of a clay after this stage has been reached. This can easily be observed by any one by drawing a piece of clay ware from the kiln, when it has attained bare redness in daylight, and cooling it promptly.

The porosity is greater than at any other stage. The mass retains the structure given it during manufacture. That is, the pores occupied by the water while the ware was plastic, still remain. To this porosity is added that produced by the loss of combined water from the clay substance, the loss of CO_2 from some of the carbonates, the burning out of the organic matter, and the partial distillation of the FeS_2. The shrinkage has not begun. The clay measures just as it did when dry. The hardness is very low—scarcely greater than that of thoroughly dried clay. Also, the strength is very low—not being much increased by the burning process so far.

The color will ordinarily have changed from the natural clay color, to a very pale yellowish-red or salmon, in those clays which contain much iron. In fact, in fire-clays which ultimately burn buff, the color at this stage is apt to be pinkish-red, which fades and yellows as the heat rises. The same is true of calcareous clays, in which the iron color is masked by the lime. These clays are red early in the burn, becoming yellow only after the lime becomes fully caustic.

The Oxidation Period.

As set forth earlier in this paper, the fundamental work of this period is the transformation of the more volatile elements of the clay from less stable to more stable forms by the absorption of gases from without the mass. This simply means that FeO or FeS must be converted into Fe_2O_3, before it is safe to proceed with the next step in the process.

The advisability of giving a separate subdivision of the whole process of clay burning to this small portion of it, may seem to some to be doubtful. Especially, when it is reflected that this stage is most important in red burning clays, less so in buff ones, and not even noticeable in white burning bodies.

But, it has been the experience of the writer, that the difficulties and losses in the burning of crude claywares are more largely referable to mismanagement of this phase of the burning than to any other; perhaps more than to all the other causes combined. And, while there is no actual line of demarkation between the dehydration, decarbonization, and oxidation reactions, all of them overlapping more or less, still it seems essential to create in the mind of the clay burner the feeling that separate and clearly marked stages exist, and that he must satisfy himself of the completion of each, before passing to the next. Lastly, while oxidation may take place at any point on the lower end of the temperature scale, it cannot take place at all if the limits of this period are once passed. Thousands of dollars would be saved annually, if clayburners could be brought to see that in passing above this temperature line, they are taking an

irrevocable step, and that clay not well oxidized at that time is practically sure to be spoiled.

It has already been hinted that the oxidation of FeO to Fe_2O_3 is not only an easy chemical reaction to perform, but even a difficult one to prevent. But also, it has heen hinted that it does not take place with facility in the burning of clays. It seems apparent, therefore, that when such an affinity as FeO and O exhibit is unsatisfied after hours of exposure to red heat in a kiln, surrounded by a constant flood of gases in which free oxygen exists, that there must be some very definite and clear-out influences which are operating against this reaction. Such is the case. There are five reasons which deserve mention in this connection.

Mechanical Action of the Clay itself. When we calcine ordinary copperas at a low red heat, we produce Venetian red, the brightest and most beautiful red which iron oxide is capable of forming. Moreover, the reaction is easy, quick and complete. But when we set free 5 or 6 per cent of FeO in a clay. we must recollect that it is distributed throughout a mass of fine particles closely packed together, which form a very effectual mechanical bar to contact of outside air.

Furthermore, we must reflect that the air can only circulate through the mass of the clay through the infinitely fine capillary system established by the water of plasticity or the almost equally fine interstitial spaces left between the grains of a dry-pressed body. We have seen that the body becomes somewhat more porous after the dehydration changes are complete, but the total amount of such porosity is, after all, not great; usually 25 to 35 per cent of the mass of the clay Into this structure, (spongy structure I had almost said, but spongy gives a wrong idea of the openness) the fire gases must penetrate to accomplish oxidation. And while they will do so in time, is it a cause for wonder that time is required? Remember, that the gas is not forced in, or sucked out, by any physical differences in pressure.

The piece of clay is merely lying in an atmosphere of flowing gases, and from this it must gradually absorb the oxygen which its interior particles crave, and must carry it

through the labyrinth of pore-spaces to the little particles of the ferric oxide waiting to receive it.

Hindrance Caused by the Evolution of Other Gases. As already pointed out, the $FeCO_3$ breaks up early, and its CO_2 should have been completely expelled from the clay considerably before dehydration sets in. But, it may happen that the temperature proceeds upward faster than the reaction can keep pace, and hence before the $FeCO_3$ of the interior is all decomposed, the other reactions of dehydration of the clay, and decarbonization of the other carbonates, are also taking place in the exterior of the mass. The CO_2 from $CaCO_3$ in particular is known to lag behind most other gases in the clay, and it is not wholly disengaged until the very time when the vitrification changes are apt to begin. This mineral therefore greatly complicates the oxidation of the iron salts on this account.

The outpouring of the steam and CO_2 from these minerals now fills the pores system of the clay. The pressure is within, struggling to get out. Hence, there is added to the mechanical difficulty of air penetrating the clay, the distinct current of other gases coming out under pressure. Naturally, therefore, oxidation is hindered until these reactions are complete.

Influence of Combustion Gases. It has been shown that FeO is prone to oxidize, even at ordinary atmospheric temperatures, when freely exposed to the air. But in the kiln, the atmosphere is no longer pure air. It is composed primarily of combustion products. The reaction of oxidation takes place, we may assume, between 400 and 900° C. It is most likely to occur between 700 and 900° C., because of the influences pointed out above. Supposing that no heat is being withdrawn for pyro-chemical processes in the wares, and that only the losses due to the escaping gases and the radiation of the kiln itself are to be accounted for, it is plain that to maintain a strong flow of gases through the kiln at a temperature of from 700 to 900° C. would require the steady consumption of a large quantity of fuel. Few people realize that the most rapid fuel consumption in the

whole burn is at just this period, and not at the end of the process, when the temperature is highest.

But if fuel must be burnt, then the air passing through must be laden with its products of combustion. If a fuel is burnt perfectly, and without excess air, its combustion products would be CO_2, H_2O, and N. If the air be in excess, O and more N are added to the mixture. If the combustion be imperfect, as it nearly always is, then the gases may contain in addition CO, CH_4, C_2H_4, CS_2, SO_2, NH_3, and H. These latter gases are all reducing in effect, and hence their effect on the process under discussion is strongly deterrent. It is remarkable that reducing gases and free oxygen may thus coexist, but analyses have repeatedly shown that such is the case.

The active value of the kiln gases for oxidising purposes depends, therefore, on several factors. 1st. The percentage of oxygen in the gases, 2nd the freedom from counteracting reducing gases, 3rd, the temperature. This field has not yet been fully studied, but the researches of T. Lowthian Bell in iron smelting have shown that Fe_2O_3 cannot be successfully reduced by furnace gas composed of CO, CO_2, and N, unless the ratio of the carbonic oxide to the carbonic acid has a certain value. The converse should be true in our case, viz., that FeO cannot be oxidized unless the ratio of the oxygen to the sum of the CO CO_2 and N has a certain value.

The lesson to be gained is, that mere exposure of clayware to the hot gases of a kiln, between 700 and 900°, while it must in time have an oxidizing effect, is by no means certain to rapidly perform the needed operation. It depends on how these gases are managed. If the fireman is acquainted with the objects of his work, he can so regulate his fires and his draft as to give his gases the utmost oxidizing value possible without loss of temperature. If he has no conception of these theoretical ends in view, he will probably make his fires work so that their gases are well nigh useless for oxidation purposes.

During the dehydration period, at least during the bulk of it, oxygen is plenty. Calculation shows that the waste

gases of the fuel must be diluted eight or ten volumes to reduce their dew point to a safe level during the drying off of a kiln. As the temperate rises, this enormous air excess decreases, but still may be three or four times what is needed for burning the fuel. But as soon as the ware is red hot through and through, the usual maxim of burners is to "push the fire as fast as the kiln will take it." They close the doors, or fill up the necks of the fire holes, and restrict the air supply, just at the very time when it is most needed.

Of course, it is evident that the air excess must be reduced, to permit the temperature to increase; but oxidation temperatures (700 to 900°) can be maintained with a very considerable air excess, if this point is duly understood by the burner, and made the subject of his skill and care.

In bad cases, when clays do not oxidize under ordinary treatment, a special procedure may be necessary to get round this difficulty. The Fish Brick Company, at Columbus, Ohio, use a bituminous pyritiferous shale, making a rather coarse-grained brick by the dry-press process, which they burn in the continuous kiln. They never have the least trouble from defective oxidation under these circumstances. But they made a few kilns by the plastic process, burning them in periodic kilns. They found, after considerable loss, that the following procedure was necessary.

The kiln was raised as usual up to about 900°C; the fires were then allowed to gradually die down to beds of ashes. The draft was reduced to a low point, and the kiln allowed to stand until the temperature had gradually fallen to 600°C, or nearly black. This took 12 to 18 hours. Then, firing up again, the kiln was carried up steadily to a finish. The secret of success in this case was the *long exposure to air nearly free from carbonic acid.* Eighteen hours of nearly pure hot air was effective, where three or four days of ordinary oxidizing at red heat had failed entirely to get the FeO all converted. Burners generally have a superstition to the effect that it is fatal to ware to allow the heat to drop back after once raising it to redness. There are good reasons in some cases, why this should be avoided. But in dealing

with such a case as this, it is practically the only way to get good results.

Deterrent Effect of Solid Reducing Agents in the Clay.

As has already been shown, clays very commonly contain organic and combustible matter. It is even added largely in many clayworking processes. Further, FeS is one of the common sources of iron in clays. These substances are all eager for oxygen and are stronger in their affinities than FeO is, Consequently, it is clear that no oxidation of the latter can possibly occur, while the former remain unoxidized.

Carbon is a constant source of trouble from this cause. Where sawdust or coaldust is plentifully added to a clay, even a well oxidized surface clay, it is quite often the case that reduction of the iron follows, and the clay shows black in the center after firing, just as if it had been a ferrous clay to start with. Still more, when a clay contains its carbon intimately disseminated through it, is it likely to prevent the oxidation of the ferrous oxide. The long and painstaking oxidation required to completely rid some clays of their carbon has already been illustrated in Fig. 3.

Sulphur is a still more troublesome substance, from the fact of its combination with iron, and consequent slowness to yield to roasting. FeS remains in black slag-like masses after the first molecule of S is expelled, and can only be destroyed by persistent and strong oxidation. Meanwhile, the iron cannot be effected, until the sulphur has been driven out.

The Effect of Structure of the Ware.

In the four preceding headings have been named the influences which are at work in the oxidation period of the burn. It is by no means to be supposed that in every clay, all of these influences are operating. In the typical brick and tile clays in use, no trouble is usually encountered, for the iron is already in the form of ferric hydroxide when it enters the kiln. Further, the clay is ordinarily sandy and open grained, and contains no sulphur and but little carbon.

Where the clay is an ordinary ferrous carbonate shale, the process is much more critical, but is still easily mastered. The hindrance to oxidation requires longer heating before vitrification, and also opener firing while raising heat. But the burner soon learns this trick, and no trouble need occur afterwards. The oxidation is here much affected by the structure of the ware. Shales ordinarily are not so very plastic. Therefore, when tempered up with water, they develop an open porous structure, favorable to oxidation.

The finer grained the clay, the more difficult does oxidation become. The writer has occasionally tested very fat, immensely fine grained clays, which do not show any grit between the teeth. Such clays will fail to oxidize and will bloat in firing, where other clays with three times as much ferrous iron will behave normally.

The fineness of grain therefore, becomes a factor of the greatest importance, and leads to practical steps to counteract its influence. Stoneware makers and sewer pipe makers have begun to find out that where they wish to make an unusually heavy piece of ware, that it is essential to open up the body of their clay by use of grog. The grog is prejudicial from one standpoint, but its aid in getting oxygen into the center of the thick ware in the same period of time as needed to reach the center of the ungrogged ware in thinner cross sections, more than over balances the detriment.

Also, brick makers have occasionally observed that an end-cut structure greatly hinders the oxidation. In one kiln made by the writer, containing about equal proportions of side-cuts and end-cuts from the same clay, the end-cuts were nearly all defective in the center and none of the side-cuts were affected. The difference comes in from the direction of the laminations, which make the center of one brick protected by sheath after sheath of clay, like a jelly roll, while the center of the side-cut is made accessible by the lamination leading into it.

Also the dry-press process and the plastic process offer some curious comparisons. In general, the latter structure is much the denser and harder to oxydize. But occasionally, a clay is encountered which is very soft and fine grained and

which packs in the dry-press die into a body of surpassing density. Such a body is exceedingly hard to oxidize, and can be worked much more safely in plastic form. But if in addition to having its iron in ferrous form, a clay contains carbonate of lime or magnesia, or pyrites, or carbon compounds, then trouble must be anticipated. The more of these materials the worse, and when all three are present together, the difficulties rise to the point of prohibiting economical manufacture.

Probably no clay exists, which *cannot* be burned safely. If it comes down to a struggle between a well equipped clayworker, and the natural perversities and disabilities of a clay, the clayworker can succeed, every time. But when the cost has to be considered, the difference between a low fuel bill and a high one; between a quick circuit of the kilns and a slow one; between a small percentage of loss and a high one; it is easy to see that some clays are better left alone. The writer knows of one instance where eighteen days were regularly required to burn a small round down-draft kiln of brick, where an ordinary Coal-measure shale should have been burned in seven. The trouble was carbon and sulphur and difficulty in securing oxidation.

Physical Structure of Clay at the End of the Oxidation Period.

The physical condition of the ware at the conclusion of the oxidation period is worth attention. If the work has been correctly done, the ware will be uniform in color in its whole cross section. Its color will be redder than at dehydration, if a red burning clay is used, or yellower if a buff clay is used. Its hardness will have sensibly increased, but it will still be soft. Shrinkage will have begun, but will have made but little headway. Strength will still be small.

On the other hand, if the clay be improperly oxidized, it will show on its fracture a spot, furthest from the exterior, of different color from the exterior of the ware. This new color is dark. The size of the spot will depend on the completeness or incompleteness of the work. The larger it is, the more certain it is to ruin the ware subsequently.

Fig. 3 very nicely shows this feature. The location of the spot will depend on the shape or position of the ware in the kiln.

In Fig. 4, the first piece on the left shows a brickette, with only a narrow border oxidized. The whole interior is untouched. But, the oxidation has penetrated about equally from all four sides. The second brickette shows the same process further along. The third brickette shows an unoxidized spot close to the left side. In this case, the brickette lay close to another, and the air got between the two with difficulty. In consequence, the protected side lagged behind the rest of the brickette.

It often happens that when bricks are faced or piled up closely on each other, that the exterior of the pile will oxidize, and the interior will not. This causes the brick to assume a fantastic appearance afterwards, one part being good and another bad, without the reason appearing from observation of the brick, apart from its setting.

In all that has been said so far, the allusions to trouble ensuing from incomplete oxidation have been frequent, but little has been said in explanation of what these troubles are. The reason for this reticence is that the real development of the trouble occurs only in the vitrification process. If the burn ended at 1000°C, then all of this discussion would be valueless. But hidden away in this apparently unimportant dark-colored spot are half the potential sources of trouble to which clays are heir; only when the vitrification of the ware is nearly finished, does this malevolent disease break out and show its capacity for damage.

THE VITRIFICATION PERIOD.

The period beginning about 900 B. C., represents the first constructive work in the burning process. All that has gone before is destructive of the original minerals which composed the clay, but not directly constructive of the homogeneous silicate, toward which the finished material is supposed to approximate.

To set forth the processes of vitrification in full detail, even if they were known, would be a task entirely beyond the scope of this present article. As a matter of fact, they are only dimly understood. The work of the cement chemists, the slag studies of the metallurgists, the work in lithological analysis by the mineralogists, all have given us something in this field, but a persevering study of the subject from the view point of the ceramist has yet to be made.

To understand the behavior of the iron in the processes which follow, we must have a little preliminary idea of the process itself. In brief, it is a continually changing series of combinations, in which more and more of the ingredients of the clay are drawn in.

The first change is thought to be a sort of condensation or shrinkage in itself of the kaolinite base of the clay. This precedes silicate formation, at least for the most part. Then the caustic basic oxides, lime and magnesia, begin to react on the dehydrated clay, forming silicates and aluminates. These soon give way to other compounds which bring the finest of the free quartz into combination. The feldspathic fluxes also begin to work, and thus from one combination to another, the work proceeds, building up one compound only to form another from it as the temperature increases and the materials present become more fully available.

If we stop the process at a number of different stages, take samples, and examine them critically, the physical structure is a warrant for the above outline. When cross sectioned and viewed under the microscope, the general truth of the above is proven, for crystalls of entirely different minerals can be identified in the same clay at different stages of its combination, and even in different parts of the same mass, where the heat has been uneven. These investigations have so far gone but a little way and the minerals formed in vitrifying clays have not yet been identified or described. We are far behind the cement chemists in this, for they have identified several minerals in portland cement

clinker, which is a much more difficult material to work with than ours, on account of its unstability.

The behavior of iron in the above process is the theme to which we must confine ourselves.

Phenomena Occurring at or before Complete Vitrification.

Vitrification among clay workers is a technical term. It does not mean, as many seem to think, that the clay has become converted into an absolutely homogeneous or glass-like solid, from which every vestige of the original mineral structure is obliterated. Such a condition is impossible in practical clay working. Even fusion does not accomplish go great a homogeneity, for when a clay is fused to a slag, its coarser particles have usually resisted solution and are still to be seen in the fluid magma.

To the practical clayworker, especially he who manufactures paving-brick, sewer-pipe, roofing tile, encaustic floor tile, stoneware pottery or porcelain, complete vitrification means that stage of a clay's burn where it becomes densest and strongest.

All clays pass in burning through a series of stages, and are progressively harder, stronger and denser up to a maximum; after that they begin to fall off, sometimes rapidly, sometimes slowly, until they finally become a worthless, vesicular, slag-like mass, devoid of useful qualities. The culminating point of excellence, though hard to define, is not hard to recognize. And while it does not by any means stand for the same qualities in any two clays, it is clearly defined to the practical man in dealing with each clay by itself.

For convenience, Wheeler* has divided the vitrification period into five stages, which mark well known differences in structure, though of course each stage passes insensibly into the next, without any dividing line. The four last stages he has named Incipient Vitrification, Complete Vitrification, Viscous Vitrification and Fusion. He gave no name to the first stage, but the writer has called it the Condensation period, as indicating that the chemical changes

* Vitrified Paving Brick, by H. A. Wheeler. Pub. Indianapolis, 1896.

occurring there are largely the simple condensation or shrinkage of the kaolinite, and perhaps other minerals, without involving actual silicate formation. Wheeler describes this stage as that of the ordinary well-burned red building-brick, which can be cut freely with a knife, and yet has shrunk considerably since dehydration.

The point of complete vitrification makes an excellent place to divide our discussion, for the reason that no one voluntarily passes it in producing clay wares. And, those changes which mature before or by that time are all familiar ones, which must be reckoned with daily. The changes which follow are destructive changes, to be avoided if possible.

The separate iron phenomena which occur before or by complete vitrification may be listed as follows:—

1st Development of the normal ferric color, red or buff.

2nd Development of the normal ferrous color, by reduction.

3rd Development of the abnormal black core, by faulty oxidation.

4th Blue-stoning, or the normal change from ferric to ferrous by increase of temperature under oxidizing conditions.

5th Reoxidation or flashing.

Development of the Normal Ferric Color. A red-burning clay which has been properly treated in the burn up to 900° C has acquired by that time a yellowish red, or pale-red color, which is called salmon. As the temperature continues to rise this color deepens and brightens, until by 1100° C, in most clays it attains its maximum brilliance and power. This red color is said to be that of free ferric oxide, which covers the grains of the other minerals with a film and creates almost as much color and as bright a tint as if the mass were ferric oxide throughout. Seger explains the successive shades of red by the increasing density of the iron oxide as the temperature rises. Condensation involves darkening of color, he argues, and cites experiments conducted on pure ferric oxide to prove the point.

The evidence that proves this to be the free ferric oxide

and not a ferric silicate, or aluminate, is more negative than positive however. It can be proven that the iron is in the ferric condition, readily enough. But to prove that it is not in combination, is not so easy. Solution of the iron is no longer possible to any agents except such as attack the rest of the clay as well. We judge by the color, which is ferric, and by the abrupt change in color which happens later on when silicate formation unquestionably does take place, that this red is that of the free oxide.

The question now arises, why is it that ferric oxide can remain so long out of combination, when surrounded by clay, silica, and other fluxing minerals, which are one by one breaking down and entering into bonds with one another? The question must remain unanswered. But experience proves that whatever the red color is, it remains intact for a long time as the heat rises, suffering no change except a gradual brightening and deepening in tint, up to a certain maximum. This maximum is not a fixed point for all clays. It varies with the composition of each. Those clays which keep it the longest are those containing least clay substance and most pure sand. A very sandy clay from Wisconsin was fired by the writer to cone 8 in a not very oxidizing burn, and remained a fine strong red, bordering on purple at this temperature. An ordinary red clay, containing 50 per cent. clay substance cannot be fired above cone 1 without beginning to show a decline. Soft fluxes like lead or alkalies, which promote silicate formation, tend to bring the iron into combination also, and destroy the red color of ferric oxide.

As a rule, the retention of the red color in its perfection and the development of a close, dense, glass-like vitrification seem to be mutually antipathetic conditions. That is, in most red-burning clays, their behavior seems clearly to bear out the contention that the color is due to *free* iron. For, as fast as the vitrification becomes visibly more perfect, the color darkens and the body seems to approach to its point of breaking down.

But occasionally, or rarely, a clay is met which contradicts this impression. Such clays have in the writers ex-

perience attained a vitrification almost like glass, preserving the while a red as bright and beautiful as sealing wax. Ink placed on the fracture, will dry and scale off without leaving a mark.

It is truly hard to see how iron can be wholly free and unattached in a silicate mass of such perfection. But, when such a clay is heated past this culminating point, it follows the general law, and blackens as it breaks down, thus seeming to show that combination of the iron means loss of its ferric character.

In one instance only, in an experience covering a large number of clays, has the writer found one which maintains a red color after passing into the vesicular stage of fusion. This clay is remarkable for immense fineness of grain, vitrification at extraordinarily low temperatures, and very early swelling into a spongy mass. It is possible that the exception is apparent rather than real, and that the clay really did blacken in fusing, but that owing to its spongy structure, and the low temperature and consequent excess of oxygen in the kiln gases, that *it reoxidized after blackening*. A supply of the clay has been secured for further tests.

A resume of the evidence, then, seems to indicate that clays are colored red by free ferric oxide, and that combination of iron with clay or silica involves blackening and formation of the *ferrous* silicate. The ferrous state of the iron in a black over-burned clay is easily proven by analysis. If this theory is not true, then the red color must be due to an unstable ferric silicate, which forms a stable black ferrous silicate after a certain critical temperature is reached. Most clays seem to favor the former hypothesis. A very few favor to the latter. It must be borne in mind, however, that these latter clays all vitrify very early, and before the ordinary range of temperatures are reached. No instance of a clay has yet come to the writer's attention which burns to a vitreous red at a high temperature.

The behavior of buff-burning clays runs parallel to the above described phenomena, except that there is much less difference between the soft and fully matured ware than be-

tween salmon and good red. The latter change is almost a change in kind, while the former is one of degree. Also, during the retention of the buff color, complete or glassy vitrification is not quite so uncommon as the sealing-wax red. But, aside from the fundamental difference in the color, the two series pass through about the same course of events, and bear out the same conclusions.

Development of Normal Ferrous Color, by Reduction. One of the strong arguments for a free ferric oxide as the basis of the red or buff color, is the prompt destruction of that color by anything like reduction. Reducing gases, smoky fire, dirty grates, or restricted air supply, very soon produce a greenish or bluish black coloration in a red or buff ware. This blue-black material is readily proved to be ferrous by analytical means. To prove that it is a silicate is not so easy. But, proceeding on synthetic lines, we can make ferrous silicates by fusion, and they all have a black color, comparable directly with that obtained in the reduced clay. When we attempt to produce a ferric silicate of a red color, if exclude the solution of Fe_2O_3 in a gloss or glaze, we fail.

The rapidity of the change from red to black depends upon (a) the violence of the reducing condition and (b) the porosity of the ware, by which the gas reaches the ferric oxide or ferric compound. Open spongy structures are affected almost at once. Dense vitrified structures suffer gradual change; sometimes several hours of continuous reduction will still fail to penetrate the center of a mass.

This reaction is exceedingly common among producers of vitrified wares. If the reduction occurred late in the burn, during the flashing or salt glazing process, it very frequently shows as a blue-black ring, extending in an eighth or a quarter of an inch from the surface, and surrounding a bright red core. Sewer pipes especially are prone to exhibit this phase, owing to the sharp pushing of the fires during the salt glazing, which is itself a reducing reaction.

Figure 5 shows several samples of this change. The piece on the left is a sidewalk paver, which has been reduced from a yellowish buff to a blue black, on one side and

FIG. 5. SEE PAGE 416.

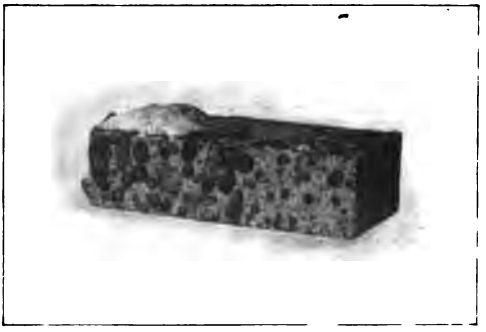


FIG. 6. SEE PAGE 418.

edge especially. The other surfaces were somewhat protected. The next piece is from a buff sewer-pipe. The interior surface shows the heaviest reduction, and the exterior surface shows a lighter, but distinct black ring. The next two pieces are parts of hollow building block, made from a clay which burns pale red. The blue-black color has proceeded half way through the piece. The piece on the right is a paving brick, of which only about one third still retains its bright red color. The blackening has gone in nearly an inch from every surface.

The permanence of such a reaction depends on the degree of vitrification attained. If the ware be porous, the reduction is ephemeral, being replaced by oxidation with equal ease. In short, while porous, the colors may be made to fluctuate with each firing, if the burner wishes. But, as the ware gains in vitrification and becomes impervious to gas flow, the colors become slower to create and slower to replace.

The effects of this reaction on the strength and value of the ware are not good. The black ferrous silicate does not seem to be as strong or durable a body as the red ferric compound. Paving brick which are thus blue-skinned, or smoked, usually wear badly for a time at least, the black crust breaking off in brittle spalls, showing the red beneath like an open wound. When shale pavers first began to be used, city engineers frequently went far beyond their province in specifying the exact color which they would require a brick to be, and many brick makers learned to put a blue exterior in a red brick to meet just this kind of demand. The results were bad and the practice has now been generally discontinued. Enough of such brick are produced by accident, without having recourse to intention.

In wares like sewer pipe, however, which have no wear to endure, this change in color and loss of strength are much less prejudicial. But, if the black color be produced, another form of the same reaction is very likely to also take place, which *is* very prejudicial indeed. I refer to the formation of bubbles, pimples, eruptions etc. on the surface of the wares.

Sewer pipe and conduit makers have no more troublesome defects to overcome than this bubbling. Paving brick makers suffer less, but face brick makers, especially those making flashed bricks, are very subject to it. No one likes to see an otherwise smooth clay product covered with little pimples or ragged-edged blisters of black slag.

The cause of this slag is the same as that of the blue-black coloration. It comes however from the occurrence of the iron in the form of concretionary grains. These masses of iron, while they remain ferric, will not readily combine with the clay. The writer has seen a case where grains of iron were still unfused in a buff face-brick burned at cone 8. The firing was done with natural gas, and the kiln atmosphere was perfectly oxidizing throughout the burn. The clay had blue stoned, but the iron had not slagged in the least.

But, let the atmosphere become reducing and these grains rapidly change to ferrous oxide. This oxide readily unites with silica or clay substance, and the result is speedily a little pool of a basic iron silicate. It is too basic to be very fluid, or to let gases escape from it or through it, so it becomes inflated and forms a little bubble. If the heat continues to rise, these in time melt, become fully fluid and finally lie flat and smooth on the surface as black velvety spots. But, if the bubble be formed at the maximum temperature of the kiln, then on cooling the wares are covered with these unsightly eruptions.

Fig. 6 shows a very bad case of this reaction on a flashed brick. The iron was excessive in amount, excessively large in grain, and the cooling happened at just the right time to show this reaction at its very worst. It is on that account instructive.

The remedy for this condition is to avoid reduction at the end of the burn. If the fires are kept clear, the grates kept clean, a certain amount of air admitted over the top of the fire, and no heavy black smoke created in firing, this pimpling on the surface, where due to iron, will not occur. With some fuels it is very difficult to avoid reduction; the choice of a proper fire place and the training of the burners

to fire frequently and but little at a time, is the remedy in such cases.

Development of the Abnormal Black Color, by Faulty Oxidation. This reaction is not to be construed as different from the preceding in so far as the actual chemical process, or the character of the black silicate itself is concerned. The reason for its description under a separate heading is merely to emphasize the causes leading up to its development, and to aid clayworkers in recognizing the phenomenon when they see it. It has already been stated that failure to bring the clay to perfect oxidation at the lower range of temperatures, 600 to 1000°C, before vitrification sets in, is one of the most destructive errors commonly made in the burning of ceramic products. It is so troublesome, because it is so insidious. No external evidence of the trouble is shown till too late. The process of burning goes along without a hitch, until the finish is close at hand, and then it suddenly shows. But then, it is long past help.

There are two reasons why a clayware which is not oxidized at the proper time is never oxidized: First, the porosity of the ware rapidly diminishes as the temperature rises. Some clays mature wonderfully quickly in the temperature range between 1000° and 1100°. The majority of red-burning glacial and alluvial clays mature in this short range. Others, the shales and the sandier common clays, require another 100° to bring them to maturity, and, consequently, still continue to oxidize with fair ease long after the close-bodied, easily-vitrified clays are past the limit.

Second. The composition of the fire gases becomes less and less fit to oxidize the ferrous oxide as the temperature increases. It has been already pointed out that free oxygen *in large excess* is necessary to rapid oxidation. But as the temperature of the kiln increases, the products of combustion will stand less and less dilution with free air without becoming too cool. As the temperature reaches 1200°, it becomes practically impossible to admit any excess air, or rather, the combustion fluctuates between oxidizing and reducing with each baiting of coal, so that the average gas produced is neu-

tral. Where temperature of cone 8 or above is used, the reducing reaction predominates.

Where for any special ware it is essential to maintain oxidizing conditions to a high temperature, 1300° or above, it is necessary either to so construct the firebox and bags as to obtain a regenerative effect on the air supply, and thus decrease its cooling effect, or to use gas fuel or some similar device. With cold air and solid fuel it is very difficult, if not impossible, to prevent occasional reduction at even 1200° C.

On these two accounts, there is but little probability of the clay oxidizing after 1000° C. is reached. In fact, but little of account happens after 900° C. Hence, for all practical purposes, we may say that when ware is not oxidized at the right time, it will not be oxidized at all.

Having now established the primary cause of the defect, let us consider its mode of operation. The central fact in this connection is that ferrous oxide combines with silica and clay at temperatures far lower than ferric oxide does. Without at this time discussing the final breaking down of a red-burning clay by excess of heat, we know that if fully oxidized and red throughout, it will stand a temperature far above that at which the black ferrous structure forms and becomes semi-fluid. It is this difference in melting point which causes all the trouble. If it did not exist, then the condition of the iron would be immaterial.

Nor is the mere fact of fusion in itself so very disastrous. The final and most troublesome fact in the whole series is the evolution of gases by the ferrous slag. It is a matter of common observation, that silicates nearly always undergo a scoriaceous stage in fusion. In glass melting this is well marked, and in order to avoid its effects the temperature of the glass is run up till it becomes very fluid indeed. After a proper exposure to this temperature, the gas evolution gradually ceases and the glass settles back into a clear, dense fluid.

The same general facts may be observed in slag fusions, or in any other silicate fusion. Clay silicates are no exception. When they melt, there is every likelihood of a considerable evolution of gases, and these swell the ware up into spongy, slaglike masses, sometimes actually so light as to

float on water. The only difference between clay and the silicates is that in the latter we aim to produce fusion, and push it far enough to get rid of the gases, but in clay we never aim to produce fusion, and consequently only carry our heating up to the very edge of this stage. Hence we encounter the very beginning of the gas evolution. In a few cases, the writer has seen clays fused completely into solid glasses, free from gas bubbles, but it is rarely that the overheating is sufficient to produce this state.

The gases which are evolved are not definitely known. There are but few which can be counted as possibilities. 1st, CO_2, from carbonates, especially carbonate of calcium, or from the gradual oxidation of carbon still inclosed in the mass. 2d, SO_2 or SO_3, from the breaking down of sulphites or sulphates, or the gradual oxidation of sulphide of iron still enclosed in the mass. 3d, H_2O, if any could still be present as part of a hydrous compound or a hydrocarbon not thoroughly coked. The latter seems improbable.

The amount of gas evolution is a variable factor. It depends on the following conditions: 1st, the amount of material in the original clay which is capable of producing gas, *i. e.*, the amount of carbon, carbonates and sulphides; 2d, the completeness with which these substances have been burned out from the clay while it was still porous; 3d, the amount of overheating after the gases begin to come off. The worst clays to deal with are those which contain carbon, carbonates, sulphides and iron, all together. Such a composition is a fatal handicap to profitable manufacture. But it must be remembered that the actual volume of the gases set free may be a very trifling amount, and still be capable of working a great injury to the ware. The gases, as soon as they are set free, expand with each increase in temperature and become worse in their effects thereby.

The time of the gas evolution depends somewhat on the clay itself. Beyond a doubt, every silicate has its own formation temperature, and before this is reached, it is innocuous. These clay silicates have not yet been studied so carefully as to enable us to say anything on this head. But, the sili-

cate formed will depend on the composition of the clay, and it is quite likely that several silicates form and dissolve one in the other, producing a slag whose melting point as a whole is referable to no single silicate. Thus a clay of fine grain and high in fluxes may blacken and swell very much earlier than another of coarse grain and low in fluxes. But in both, the same general relation between the blackening of the ferrous core, and the breaking down of the oxidized exterior seems to prevail. That is, the red oxidized portion of one clay may break down from excess of heat, before the black core of another clay may begin to swell, showing that black coring is a phenomenon which is consistent with its own clay, but not comparable, clay with clay. In general, the defect becomes evident just prior to the normal finishing of the kiln. The red portion of the ware is usually approaching its best strength when the black portion becomes so scoriaceous as to attract attention by the change in shape

Allusion has been made incidentally in many places in the foregoing pages to the effects of this gas evolution. In brief, they involve a swelling or bloating of the black ferrous portion, and a sufficient stretching of the surrounding red portion to accommodate the change in volume. Naturally the extent of this bloating depends on the amount of gas liberated, the increase in temperature after liberation, and the structure of the red portion of the body. If the latter is porous, the swelling will be slight or unnoticeable. If it is dense and impervious, the swelling will be very severe. The action is exactly that of yeast in a loaf of bread, except that it proceeds from a central focus instead of throughout the structure equally..

The practical range which this troublesome defect may take in claywares is very great. Figure 7 shows two bricks split longitudinally, illustrating a very bad case of deformation. The red ferric shell is about one-half inch in thickness on the sides, thicker on the ends and corners. The black spongy core has generated such a volume of gas that it has created large cavities between it and the exterior. These bricks were made from a carbonaceous, pyritiferous, ferrous

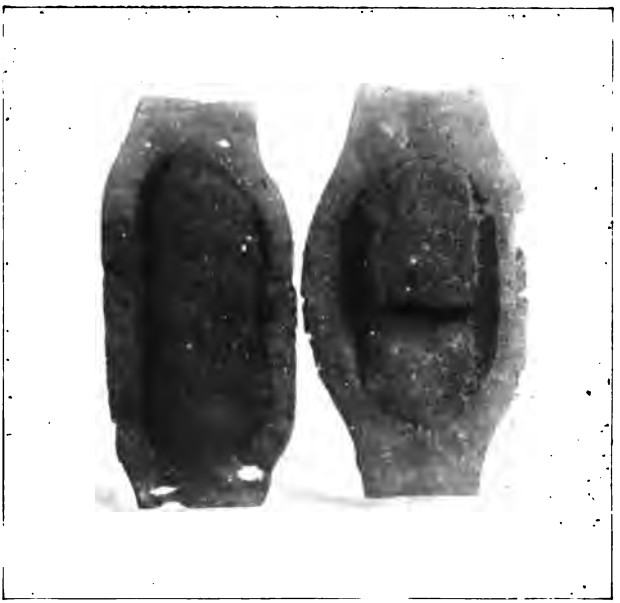


FIG. 7. SEE PAGE 422.

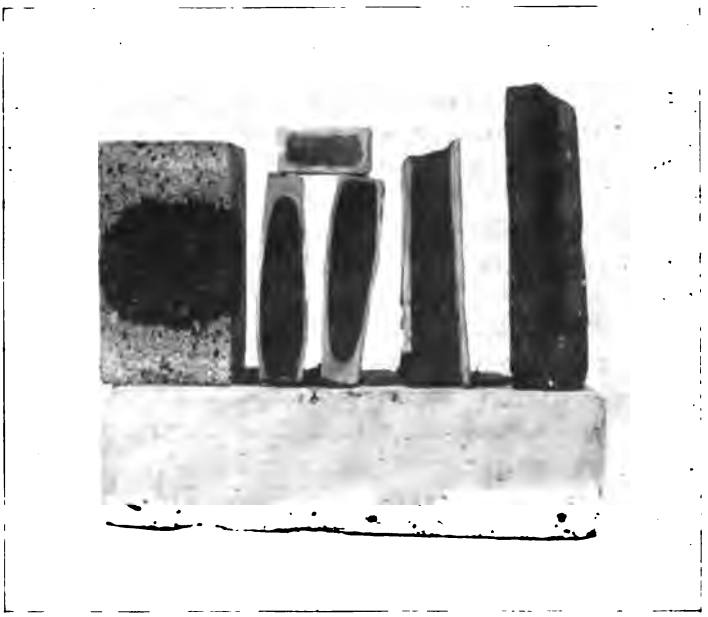


FIG. 8. SEE PAGE 423.

shale, by the stiff-mud process. Even this does not represent the maximum. Cases have come under observation where the clay has bloated up to three or four times its original volume.

A more moderate phase of the defect is shown in Fig. 8. On the right is a piece of red sewer pipe. The blue core is here about one-third of the cross section, and the gas evolution has been small. Consequently, the ware is not swollen; but it is weak and brittle. There seems to be a very distinct difference between the physical properties of the blue and red, because the ware is invariably weakened by a blue core, even when the vesicular structure is microscopic. This constitutes one of its chief disadvantages; swollen wares can be picked out and discarded. But when they are merely blue-cored, their unsoundness only develops by use. After the failure has come, we see why it came. But that does not guard against its recurrence.

The piece next the right is a beautiful example of change in color, without swelling. It is from a drain tile, made of a tough fine-grained alluvial clay, which would ordinarily speaking be called perfectly safe from such defects. The piece was burned in about 24 hours, which explains the trouble. The two small pieces next the left, are trial pieces burnt by students. They show the contrast in a buff clay very prettily, and also show slight swelling. On top of them, is another small trial piece, in which only a faint blue core shows—no swelling. The brick on the left shows a dry-press product, made from a fire-clay containing some carbon. It has not swollen, but the brick was so weak as to be rejected from use. In other instances, this defect may represent merely a faint blue line or spot in the thickest and best protected portion in the ware. In such cases, its power to do harm is small.

Summarizing, we may say that failure to oxidize the clay to the center at the proper time, usually results in the failure to oxidize later. The ferrous oxide thus left forms a fusible silicate at a temperature when the well oxidized exterior is only becoming well vitrified. This fusible silicate evolves gases, in greater or less volume, which tend to swell the

ware. In cases where the amount of fusible silicate is small or the gases are not abundant, the discolored core does not swell, but weakens the product by unequal expansion and contraction.

Blue Stoneing, or the Normal Change in Color by Increase in Temperature. With this phenomenon, every clayworker and many ordinary observers outside of the clay industry are familiar. With its cause, few if any can claim knowledge. The writer acknowledges that it has gone no farther than speculation with him.

The brick shown in the bottom row in Fig. 9 are red burning paving brick. All are thoroughly vitrified in the usual sense that clayworkers use this term. None of them would absorb more than a fraction of a per cent. of water. But the left hand one is red as liver, the middle one is a dark red, almost a chocolate, and the third is of a purple or violet color. In the upper row, on the left, is a buff fire-clay brick; next, the same changed to a soft blue-gray tint, and on the right a darker purple-gray. None of the six samples are in the least swollen or vesicular. None are reduced by gases working on the interior or exterior. They have simply changed color by increase of temperature, being maintained all the while in oxidizing conditions.

It is easily possible to overburn either of these clays, but in the ordinary acceptation of this word, these clays are not overburned. Their strength and hardness and density are about uniform throughout the series. A small increase of temperature would bring an abrupt and marked physical change in both of the right hand specimens, which will be described later on.

What, then, is this mysterious change in color. The writer believes it is the preliminary stage of that breaking down by which the ferric oxide, or ferric compound, whatever it may be, is converted into ferrous oxide by the superior energy of the silica and formed into a ferrous silicate. This reaction, if there is such a one, is brought into full completion during fusion, and its further discussion will be deferred till that topic is reached.

The alternative proposition to which we are driven is

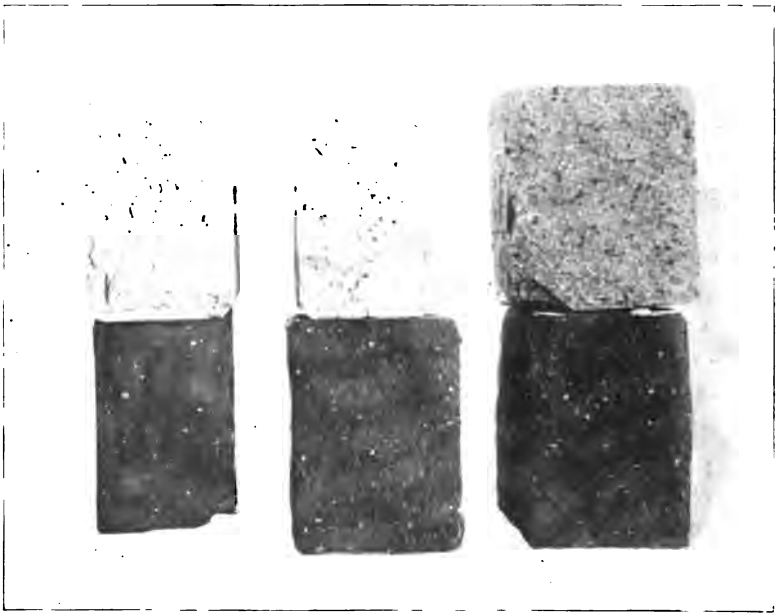


FIG. 9. SEE PAGE 421.

that advanced by Seger, who thinks that the increasing density of the ferric oxide as the temperature mounts higher, is sufficient to explain all these shades between light red or buff and violet black. He does admit that ferric and ferrous oxide, in different proportions, may cause a wonderful variety of tints. No one would oppose this statement. But, the question arises, how could a densely vitrified mass of clay like a brick, being heated in purely oxidizing atmosphere, generate any such mysterious mixture of ferrous and ferric oxide, after it has once safely passed the oxidizing period?

Whatever its cause, all clays show this phenomenon to a greater or less degree. Sometimes, as in the limy clays, the transition from vitrification to fusion is very short. But in its traverse, however short or long it may be, this mysterious, delicate change of color comes. It must not be confounded with any of the oxidation or reduction phenomena discussed heretofore.

Reoxidation, or Flashing. This topic has already been discussed by Bleininger and others, and will therefore receive but a word here. It has been shown that where a clay is frequently subjected to alternate oxidizing and reducing conditions, that it shows a change in color, which is superficial if the ware is vitrified and dense, and entire when the ware is porous. This color is that produced by ferric oxide, only darker and stronger. In a buff clay, it is golden, russet or brown. In a red clay, it is chocolate or almost black.

It has been shown that this color, cannot be developed by a continuously oxidizing burn. It requires reduction, *followed by oxidation*, to develop it. The more times this change occurs, the more brilliant the color and the easier it is to develop. It also requires that the reoxidation shall be as well marked as the reduction. A cooling under reducing conditions, or a very short or sudden cooling, leaves the ware in its reduced or ferrous color.

These facts are now well in hand and of commercial use among the numerous class of clayworkers who are producing flashed brick, and similar wares. I propose to dwell for a moment on another phase of the subject which is never pro-

duced with intention, which is counted a defect, and which is generally not comprehended by those who encounter it.

The two bricks shown in Fig. 10, were made for buff bricks. But in the center of one, and on the side of the other, is a zone of distinctly pinkish color, quite widely different from the clear cream color of the rest of the ware. These pink spots were reduced in the burn, but not by gases and under the burner's control, as in flashing. They were reduced by the carbon in the clay itself, which was in a way fixed or "set" in the clay by too sudden heating. It is not unlike the way that a dye sets in a fabric under the influence of a mordant.

Carbon, especially that of the bituminous type, is very difficult to handle. It is apt to catch fire, and raise the heat too fast. Or it is likely to decompose, or "crack," and deposit a fine coke in every pore of the clay, where it can only be burnt out with the extremest care and plenty of time. But, by slow firing, by cutting off the air supply, by use of grog or grit in the body, and similar means, this may be overcome or avoided.

If carbon is once allowed to remain in the clay till a good red heat is reached, then its effect is inevitable. It will reduce the iron. If this be carefully reoxidized the black core may be obviated. But, the reoxidized iron will no longer take the same color, as the exterior shell. It takes the redder, stronger tint instead. Thousands of face brick go for seconds every year on account of this defect, which could be avoided by a little more care or a little more understanding of this simple fact.

Phenomena Occurring after Vitrification or by Reason of Excessive Heat.

The natural end of the heating process is fusion. But clayworkers only push their process to this extreme by accident. In the very large majority of cases, the substance produced by the fusion of the clay as a whole, is distinctly refractory. That is, it does not melt readily, compared to slags, glasses, and other artificial silicates. On this account,

clays generally pass from vitrification into fusion by slow and insensible stages. This quality is very necessary to satisfactory clay-burning. Where the temperature interval is short, as in the limey clays, they become very "unsafe," *i. e.*, the burner must pay the penalty of the least excess of heat, with a lot of fused and worthless ware.

The stage known as viscous vitrification is the form of overburning shown in the great majority of cases. The limits of this state are defined, first, by the point where the clayware is no longer able to resist the pressures which it has to endure under normal kiln conditions; second, when the ware is unable to endure its own weight and sinks out of shape by gravitation alone.

In dealing with different products the setting is managed on so widely different plans that normal kiln pressures are not comparable. A paving brick carrying the weight of thirty superincumbent courses is certain to sink out of shape far earlier than a piece of pottery in a saggar. Therefore viscous vitrification does not mean the same stage of fluidity in different products.

In no clay do the whole mass of the minerals arrive at fusion simultaneously. In vitrification we must conceive of a mass or skeleton of rigid unfused minerals greatly in preponderance, permeated by a small proportion of fused silicate matter. This latter is enough to tie the other materials together, to render the mass harder and stronger, but not to cause it to lose its shape. But, as the process goes on, the proportions of stiff rigid matter and fused matter are gradually reversed. The point at which a ware yields to kiln pressure and deforms, therefore, depends (*a*) on the proportion of coarse-grained relatively infusible, insoluble minerals which will act as the skeleton or frame to resist change of shape; (*b*) on the kind of a solvent fluid slag which is attacking this skeleton and undermining its power to resist; (*c*) on the pressure or weight to which the ware is subjected.

The nature of the breaking-down process has been discussed already, in connection with black coring and bluestoning. The exact nature of the chemical compounds which fuse first, and thus lead to a fusion of the clay as a

whole, are not known. It is not likely that they are the same in many cases. Or, rather, that the total number of minerals which may form in a fusing clay is large, but that the proportions of each in any given case are not likely to be duplicated by any other case. It is probably a case of solution of one silicate in another, and in many cases we are not likely to resolve the question of proportion in the near future or by existing means.

There are two facts in connection with the breaking down of a clay by heat which are noteworthy, viz., the developement of the vescicular structure, by gas evolution, and the change of color. These two phenomena are not invariably manifested, but only in very rare instances do they fail. The clays which fuse most quietly are white-burning bodies using lime as a flux. The writer does not recall a red- or buff burning clay which fused without gas evolution.

The explanations given of gas evolution in the formation of the ferrous silicate at lower temperatures hold good here only in part. The chief difference is in the quantity and nature of the gas-forming substances still left in the clay. Water and probably CO_2 are now ruled out. The clay having been thoroughly oxidized and thoroughly vitrified, is supposed to contain nothing further combustible or readily volatile. There are two other sources of gas remaining; first, SO_3, from the breaking down of sulphates, which are known to resist decomposition much more tenaciously than carbonates or any other salts found in clays; second, oxygen, derived from the breaking down of Fe_2O_3 to $2FeO$, before passing into silicate combination.

The Vesicular Structure Produced by Sulphates. Beyond a doubt, this swelling is a common and important cause of the breaking down of clays. In Fig. 11 are shown two cases. The fireclay brick has become minutely cellular, and has increased in all dimensions about equally. Fusion has not set in; viscous vitrification is admirably represented. But the brick has lost its hardness, strength and density to a considerable extent. Its color has not materially changed. This seems a typical case of sulphuric acid swelling.

The red brick is a dry-pressed ware made from a pyrit-

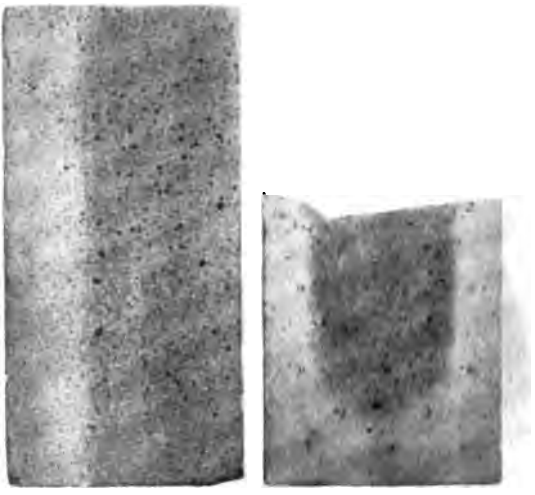


FIG. 10 SEE PAGE 426

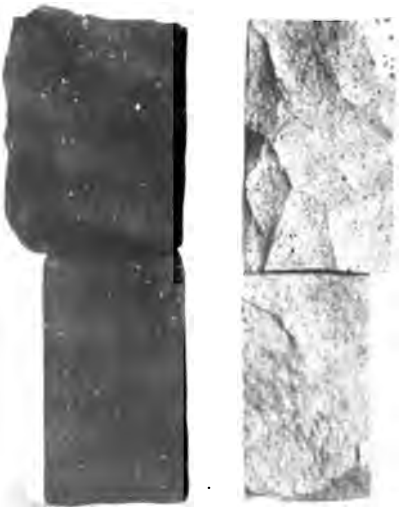


FIG. 11. SEE PAGE 428.

eferous clay. It has swollen much more, and less regularly than the other. It is fused, rather than in the stage of viscous vitrification. It also has lost strength, hardness and density. Its color has changed somewhat, *but is still red.* The entire absence of darkening in the center proves that the clay has been fully oxidized. Moreover, its heating has been but little above the point of complete vitrification. The other end of the same brick was sound. This also seems a clear case of sulphuric acid swelling.

The Oxygen Hypothesis. The two cases above have been picked out as unusual. Ordinary claywares blacken extensively in fusion. And their swelling occurs with and in proportion to their blackening. This seems a significant fact. While red or buff, analysis demonstrates their iron to be ferric. But after this breaking down and blackening, it shows it to be ferrous. This change has occurred progressively from outside to inside, as the temperature is taken up from the gases surrounding the ware.

It is hardly possible to explain this sudden change by reduction by gases, which we know operates so frequently at lower temperatures. This might happen slowly and gradually, if the gases were persistently kept reducing. But how could these gases penetrate a dense vitrified structure and reduce its iron in a few minutes? And yet a few minutes of overheating is enough to cause the change.

The writer has long held the opinion that this change in color and in structure has its explanation in the breaking down of the ferric oxide, into ferrous oxide, with evolution of oxygen. This theory accords with all observed facts, so far as they are at hand; its sole weakness is the hypothetical breaking down of the Fe_2O_3 without the aid of a reducing agent. This has seemed improbable to many to whom it has been suggested. But there are many other instances of oxides breaking down by heat alone. If this hypothesis is ultimately proved to be true, it only adds another oxide to the list.

It will lead too far to discuss the whole question of the existance of ferric silicate at this point. But we know at least, that ferrous silicates are easily made synthetically and

that ordinary technical and industrial work does not offer similar illustrations of ferric silicates. The breaking down of the iron under stress of high temperature, and the action of silica in immediately forming the black silicate, seems to more adequately explain the observed facts than any other theory yet advanced.

In the preceding pages, the writer has marshalled the principal facts concerning the behavior of iron in clay burning as they have appeared to him. In closing, he wishes to repeat the opening remarks, to the effect that there is much more of opinion than research in the paper. If it suffices to show the very numerous gaps in our exact knowledge of this important and fascinating subject, and inspires in the mind of any the desire to unravel any one of these problems, it will have answered its purpose well.

NOTE BY THE SECRETARY.

The foregoing paper was delivered at the Boston meeting in the form of an address. Since that time, it has been extended and considerably changed in method of presentation. In view of these changes, it has been thought best not to print the discussion which occurred after the address, but to specially request those who spoke at that time to formulate their views in detail and send them in in writing for publication at a later time.